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Editorial

SCIENCE AND WITCHCRAFT

The U. S. Department of Agriculture said in 1898 (Farmers' Bulletin No. 79):

"The fertilizer industry in this country has grown to these proportions within fifty years. It was but natural that a business growing so rapidly and offering such opportunities and temptations for fraud and imposition should have early become a field for extensive operations of unscrupulous and dishonest men. Fraud became so prevalent as the industry developed in one State after another that there was an urgent demand both from the consumers of fertilizers and from honest manufacturers and dealers in fertilizers for laws providing for the inspection of fertilizers with a view to the prevention or the detection and punishment of fraud."

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"The man who peddles a worthless 'formula' at an exorbitant price or exploits under a high-sounding name a mixture which has little or no value as a fertilizer is still abroad in the land."

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"There are two claims which generally characterize the representations of the companies and agents selling these questionable goods:

(1) The process of manufacture is a secret one, having been 'discovered' by some one who is generally unknown either to science or practice.

(2) The 'fertilizer,' or 'food,' either contains ingredients of which the whole world, outside of a favored few, is ignorant, or else certain ingredients are so wonderfully compounded as to produce marvelous results."

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A Federal Trade Commission hearing examiner, in a 1956 decision, said:

"From the record as a whole, it appears that we may here be confronted with a device operating upon a principle unknown to or unrecognized by present-day science. The strongest indications of this possibility lie in the scientific testimony in support of the complaint, wherein the scientists admitted that they did not understand the theory upon which the Evis Water Conditioner purports to operate."

". . . we must not take the risk of interfering with the development of a device which may prove to be the first practical application of a scientific principle heretofore undiscovered."

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One of the tests of the Evis Water Conditioner consisted in washing eight identical glasses in warm soapy water. Four were rinsed in "treated" water, four in untreated water, and allowed to dry. Two persons examined the glasses and picked the four dirtiest and the four cleanest. Two of the dirtiest had been rinsed in "treated" water, two were controls; and, of course, there was the same division among the cleanest glasses. The examiner said, "It appears that the tests . . . as to water stains prove nothing, because the negative and positive thereof were exactly equal."

We wonder what would have happened in 1898 if a "fertilizer" manufacturer selling sand and clay (with plant food so cleverly compounded as to defy chemical analysis) had submitted to a plot test and claimed that since half of the treated plots grew better than the control average, it proved his product worked half the time, even if it was not effective on all soils!

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In the Scientific Monthly, May 1953, an article on "The Involuntary Destruction of Science in the USSR" states:

". . . when the final authorities who control science are not scientists themselves, sooner or later quacks will flourish and ultimately dominate the field. Even with the very best intentions, those who are not scientists cannot decide scientific questions and cannot, in the last analysis, even choose between scientists and charlatans. It is only when science is free and autonomous, when enlightened scientific opinion is the court of last appeal, that impostors can be exposed as they arise. Otherwise quackery will flourish, for the plausible over-simplifications of quackery have always had a great popular appeal."

BACTERIOLOGY.—*Pertussis and pertussis vaccine control.*[1]

MARGARET PITTMAN, National Institutes of Health.

There is a tradition, that after the president of the Washington Academy of Sciences has fulfilled his responsibilities and relinquished the office, he emerges from retirement one month later to address the members. The selection of a subject, that might be of interest to the membership, which represents so many varied fields of science, was not easy. Recently one of the little known activities of the government, that function behind the scenes, was suddenly brought to the attention of the people in connection with the control of poliomyelitis vaccine. Because of the current concern, it was thought that a talk about the control of another product, pertussis vaccine, and the apparent effect of control on the death and case rates of pertussis, commonly known as whooping cough, would be of some interest.

The Federal control of interstate shipment and export and import of biologic products began July 1, 1902, with the enactment of Public Law 244, commonly known as the "Biologic Law." This law was consolidated with other laws relating to the Public Health Service in 1944, in Public Law 410, now known as the "Public Health Service Act." The original law in essence provides that certain biological products may be prepared only by inspected and licensed laboratories and that appropriate regulations must be promulgated to control the safety, purity, and potency of such products.

The need for the control of safety was emphasized in 1900 by the tragic death of a number of children in St. Louis. The children died from tetanus. They had been ill from diphtheria and were treated with antidiphtheria serum. It was shown later that the serum had been collected from a horse in the incubation stage of tetanus and that the serum contained tetanus toxin.

The functions of the biologic control law are carried out by the Division of Biologics Standards, until recently the Laboratory of Biologics Control, of the National Institutes of Health under the administration of the Secretary of the Department of Health, Education, and Welfare through the Surgeon General of the Public Health Service.

This evening, I shall discuss the relative importance of pertussis as a communicable disease of childhood and the possible influence of the establishment of a standard of potency for pertussis vaccine, on the accelerated decrease in death and case rates that has followed.

PERTUSSIS MORTALITY: COMPARISON WITH FIVE COMMUNICABLE DISEASES

Pertussis has ranked among the topmost highly fatal communicable diseases of childhood, yet it has not claimed as much attention as some of the other acute infectious diseases. It has been lightly regarded by the public and even by some physicians (cited by Gordon and Hood, 1951). Perhaps it was because people saw the relatively mild cases in the older children of school age but seldom the baby sick in bed. It was one of the communicable diseases that children were expected to contract sooner or later.

In Fig. 1 is shown the yearly death rates, per 100,000 population, of pertussis from 1900 to 1954 inclusive, in the United States Registration Area, in comparison with the rates for diphtheria and measles. The statistics are based on reports that vary in completeness, not only from one State to another but within a State. Records from cities as a whole have been more nearly complete than those from rural areas. In the early part of the century, a number of States were not included in the Registration Area. All but two were included by 1925. The data, however, serve as a basis for determining the relative positions of the diseases.

There has been a marked decline in the death rates of each of the three diseases. The most rapid occurred with diphtheria; from 1900 to 1925 there was a significant decline while the death rates for pertussis and measles, which were comparable, remained relatively stable. After 1925 there were marked declinations in the death rates of both per-

[1] Address of the Retiring President of the Washington Academy of Sciences, presented before the Academy on February 16, 1956.

tussis and measles but somewhat less for pertussis than measles until 1951 when the death rates of pertussis fell below those of measles. Since 1934 the death rate of pertussis has exceeded that of diphtheria. It should be noted that the most rapid decline in the pertussis death rate began after 1943. In the next ten years the decline was greater than one log, more than had occurred in the 43 years previously.

Pertussis is singular among the communicable diseases in its marked predilection for attack of infants and young children and for a higher death rate in the first year of life. The death rates, per 100,000 population, for the three youngest age groups, from 1940 to 1954, inclusive, are given in Fig. 2. Rates for the 10 to 14 age group were omitted; they did not exceed 0.1 and have been below 0.1 since 1947. During the 15-year period, the death rates for the under 1-year age group ran about one log above those for the 1 to 4 year group and more than two logs above those for the 5 to 9 year group. The steep decline in death rates for all ages since 1943, noted in Fig. 1, is also evident here for each of the age groups, especially for the two youngest age groups. For the under 1 year of age the decline was from around 100 to 7.5 and for 1 to 4 years from around 10 to 0.7.

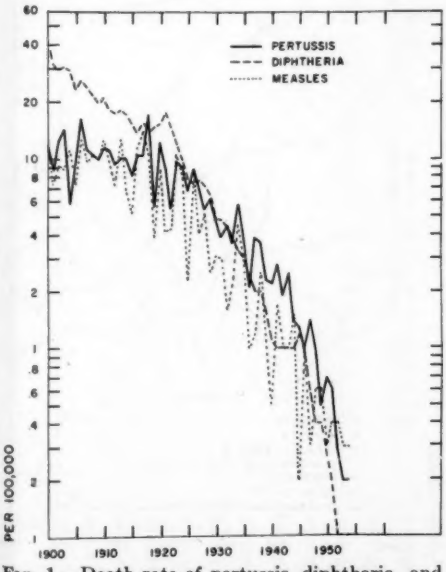

FIG. 1.—Death rate of pertussis, diphtheria, and measles, U. S. Registration Area, 1900 to 1954.

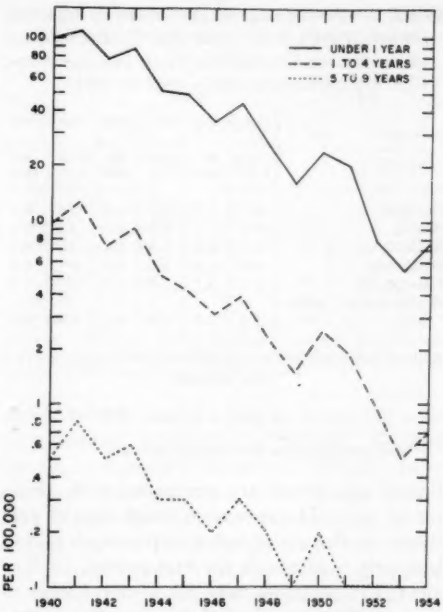

FIG. 2.—Death rate of pertussis by age, U. S. Registration Area, 1940 to 1954.

Since the greatest number of deaths caused by pertussis occur in children under one year of age, the relative importance of pertussis as a communicable disease for this age group can be observed only by comparing the rate with those of the other diseases for the same age group. In Fig. 3 is shown the yearly death rates, per 100,000 population, of six communicable diseases for the under 1-year age group for the years 1940 to 1954, inclusive. The curve for pertussis is the same as the one shown at the top of Fig. 2. During the 15-year period there was a marked decline in the rates for diphtheria and a lesser decline for measles while there were no significant over-all changes in the trends for scarlet fever and streptococcus sore throat, poliomyelitis, and meningococcus infections. Although there was a rapid decline, the pertussis rate remained above those of all of the other five diseases until 1952 when it fell below the meningococcus infection rate.

In table 1, 5-year mean death rates of the six communicable diseases for the under

TABLE 1.—FIVE-YEAR MEAN DEATH RATES PER 100,000 POPULATION FOR SIX COMMUNICABLE DISEASES FOR UNDER ONE YEAR AND ALL AGES, U. S. REGISTRATION AREA: 1940 TO 1954

	1940 to 1944		1945 to 1948		1950 to 1954	
	Under 1 yr.	All ages	Under 1 yr.	All ages	Under 1 yr.	All ages
Pertussis	84.7	2.2	33.8	1.0	12.9	0.4
Measles	16.2	1.1	6.5	0.5	3.9	0.3
Scarlet fever*	1.1	0.3	0.6	0.1	1.0	0.2
Diphtheria	4.4	1.0	2.2	0.7	0.4	0.2
Poliomyelitis	2.0	0.7	2.0	1.1	2.3	1.2
Meningococcus infections	9.0	1.2	8.9	0.8	9.8	0.7
Ratio of death rate of pertussis to the sum of death rates of 5 other infections	2.6	0.5	1.7	0.3	0.7	0.15

* Streptococcus septic sore throat is included.

1-year age group are compared with those for all ages. The excessive death rate of pertussis for the young age group in each period is clearly evident. In the first period, 1940 to 1944, the pertussis rate for under 1 year of age was 2.6 times that of the sum of the other five diseases; 10 years later, however, it was only 0.7 of the sum. In the first period the pertussis rate for all ages ranked first: From almost twofold to more than sevenfold greater than that of the other five diseases. Ten years later it ranked third: Below meningococcus infections and poliomyelitis death rates and not more than twofold greater than the rates of any of the diseases. The ratio of the pertussis death rate for all ages to the sum of the rates for the other five diseases declined in the same magnitude as for the under 1-year age group, from 0.5 to 0.15. Declination for both groups was steeper between the periods 1945 to 1949 and 1950 to 1954 than between 1940 to 1944 and 1945 to 1949.

MORBIDITY

In 1943 Dauer summarized the reported case and death rates of the States for 1925 through 1939 and for seven States and two cities for 1910 through 1939. It was shown that there was a rise in the rates of the reported cases after the death rates had started to decline. This was attributed to improved reporting of cases rather than to an actual increase in incidence. The number of cases per death gradually increased from an average of 10 cases per death in 1910 to approximately 100 for the 5-year period 1935 to 1939. For the seven States the highest case rates occurred either in the 5-year periods 1930 to 1934 or 1935 to 1939, while the highest death rates occurred in the periods 1910 to 1914 or 1915 to 1919. The case and death rates for three of the States, to which we have added the rates from 1940 to 1954, are shown graphically in Fig. 4. There was no significant change in the case rates until after the period 1940 to 1944, at which time a marked decline started. At the same time the slope of the curve of the death rates became steeper. In Fig. 1 it was shown that the highest death rate for any single year from 1900 to 1954 in the total Registration Area was in 1920, and that after this date there was a trend downward which was accelerated after 1943.

The data for the total Registration Area given in Table 2 show a similar decline in death and case rates as was demonstrated in Fig. 4. The decline in mortality began after 1920 and was accelerated after the 3-year

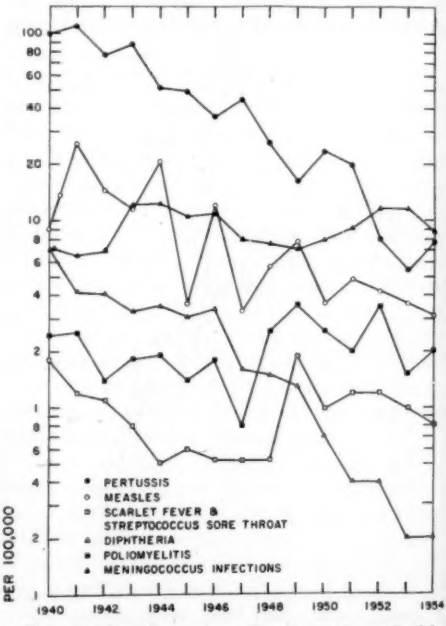

FIG. 3.—Death rates of six communicable diseases for under 1 year of age, U. S. Registration Area, 1940–1954.

period 1943 to 1945. The decline in case rates started after 1940 and was accelerated after the period 1943 to 1945. In the interval between the 3-year periods 1943 to 1945 and 1953 to 1955 the case rate declined from 109.1 to 33.5, a decline of about 70 percent. During the same period the death rates declined from 1.7 to 0.2, a decline of about 90 percent.

Another illustration of the morbidity decline is shown in Fig. 5. The actual number of cases reported, not the rates, for the past 18 years are given. During this period the susceptible population was almost doubled by the increase in births during and since the war. The reported cases of poliomyelitis are included for comparative purposes.

No discussion about pertussis would be complete without mentioning the sequelae, especially in infants, that may follow recovery. The most important one is neurological damage which may be manifested by retardation in learning and in behavior disorders (Levy and Perry, 1948; Schachter, 1953; Annell, 1953). Schachter found that two-thirds of 200 young children following noncomplicated pertussis in infancy showed various character disorders. Annell, in a study of the more than seven hundred children, found that emotional instability, delinquency, and other behavior disorders occurred more frequently in those who have had pertussis than in those that had not had the infection.

TABLE 2.—PERTUSSIS DEATH AND CASE RATES PER 100,000 POPULATION, U. S. REGISTRATION AREA

Years	Death		Case	
	Rate	Reduction	Rate	Reduction
		Percent		*Percent*
1908–10	10.8			
1918–20	11.7	0		
1928–30	5.5	53	143.7	
1938–40	2.7	51	151.6	0
1943–45	1.7	37	109.1	28
1948–50	.7	59	59.3	46
1953–55	.2*	71	33.5	44

* Estimated death rate for 1955, 0.2, the same as 1954; Number of cases in 1955 was the same as in 1954.

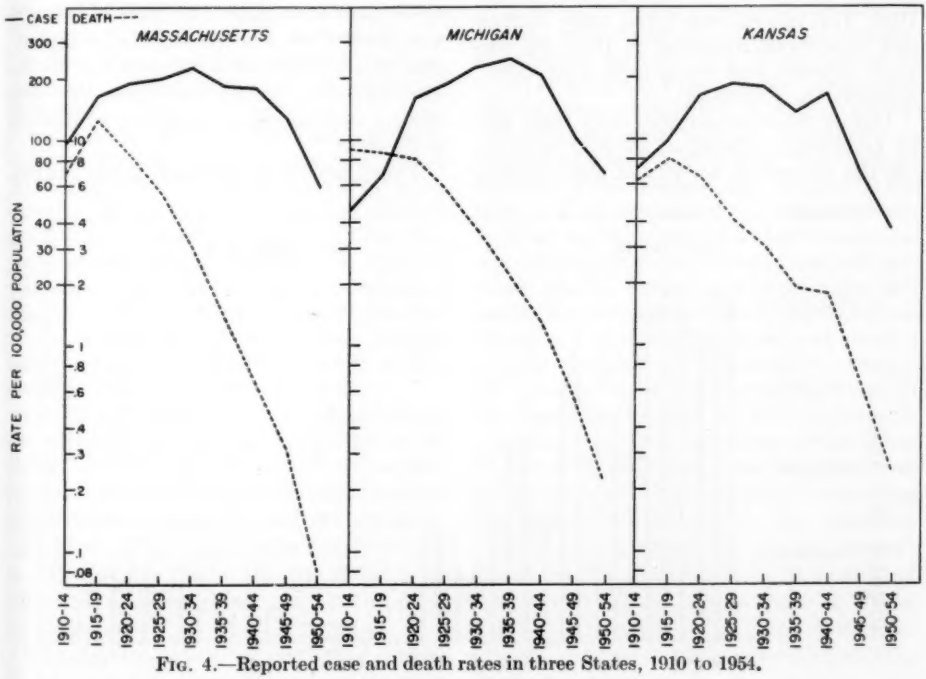

FIG. 4.—Reported case and death rates in three States, 1910 to 1954.

PERTUSSIS VACCINE

The pertussis bacterium, *Bordetella pertussis*, was not isolated until 1906, after many attempts had failed. Shortly afterward vaccines were prepared and used both to prevent the disease and to treat it. In the earliest available records of federal licensed products, January 1914, pertussis vaccine is listed. The Council of Pharmacy and Chemistry of the American Medical Association admitted pertussis vaccine to New and Nonofficial Remedies in 1914. Results from the use of vaccine, however, were so unsatisfactory that the Council omitted pertussis vaccine from New and Nonofficial Remedies in 1931. In the same year a significant contribution was made to the knowledge of the organism which enabled workers to select better strains of bacteria for the preparation of vaccine (Leslie and Gardner, 1931). Within the next few years there appeared several papers that presented convincing evidence that pertussis vaccine could be effective in protecting children against the disease. On the other hand, failures to obtain significant protection continued to appear through 1945. The vaccine was listed again in New and Nonofficial Remedies in 1944. By this time vaccines were being used quite extensively in the United States.

During the controversial years there was no satisfactory laboratory method to measure the protective activity of the vaccines, hence no U. S. Standard of Potency. We had to wait until someone discovered how to fatally infect a mouse with *B. pertussis*. Different workers tried the usual test tube and animal protection tests to measure the antibody response of animals. The former included agglutination, precipitation, complement fixation and opsonocytophagic index determinations. These tests showed that the animals had responded by producing antibodies to the antigens injected but there was no definite assurance that the antibodies were protective.

The protective tests in laboratory animals were not satisfactory because of inability to produce a fatal infection in laboratory animals. Mice could be killed if they were injected intraperitoneally with about 1,000 million organisms but death was largely caused by toxicity and not infection. The slight amount of protection that could be obtained was antitoxic and not antibacterial in nature. Since pertussis is a respiratory disease, attempts were made to induce infection by injecting the culture intranasally. Infections were induced but very few mice died except when near toxic doses of bacteria were used. A few workers reported some success with both routes of injection but no one was successful in standardizing the procedures with reference to potency evaluation of vaccines.

A new method of approach to the evalua-

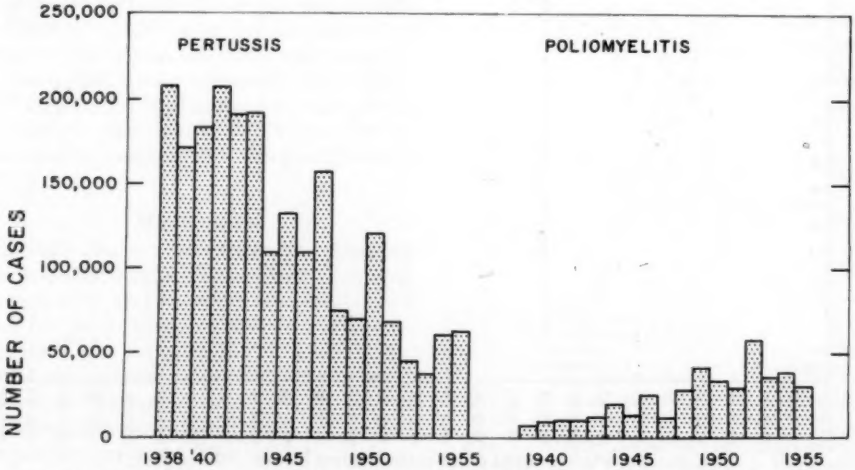

FIG. 5.—Reported cases of pertussis and poliomyelitis in the United States, 1941–1955.

tion of pertussis vaccine was opened in 1943. Dr. John F. Norton, of the Upjohn Company, in some protection tests, tried infecting mice by the intracerebral route. The results were very encouraging. He did not publish his findings but reported them, in 1944, to the Laboratory of Biologics Control and Dr. Pearl Kendrick, of the Michigan State Laboratories. I had just started on the problem of developing a U. S. Standard of Potency. Dr. Kendrick with Dr. Grace Eldering had previously carried out some successful field trials of pertussis vaccines. They were at the time actively engaged in studying protective test. Through close cooperation rapid progress was made towards the development of a protective test. In March 19, 1945, the National Institutes of Health notified the manufacturers of pertussis vaccine of the promising results and asked them to carry on independent investigations so that more data could be accumulated. That fall the majority of the manufacturers collaborated in a study under the direction of Dr. Kendrick to determine if comparable results could be obtained in different laboratories. The manufacturers were very anxious to have a standard potency test and through the American Drug Manufacturers Association were contributing toward the support of this study.

On January 5, 1946, the National Institutes of Health issued "a tentative mouse protection test for determining the antigenicity of pertussis vaccine." This test later became the official test. It differs from the one used in the collaborative study in that mice were given only one immunizing dose of vaccine instead of two. Experience in our laboratory had shown that reliable results could be obtained with one dose. This shortened the test from 33 to 28 days.

On May 27, 1948, minimum requirements for pertussis vaccine were issued to be effective January 1, 1949. This interval permitted time for the manufacturer to make adjustments in production and control methods. Most of the manufacturers, however, had already made such changes as had been indicated, and were before 1948, testing each lot for potency.

The need for potency evaluation of pertussis vaccine was demonstrated in the early part of our study. In 1945 and 1946 we tested samples of vaccines from fourteen manufacturers in the United States. It was found that the bacteria in lots of vaccines from three manufacturers had no detectable potency while those with demonstrable activity, from other manufacturers, showed as much as 10 fold variation not only between manufacturers but between lots of the same manufacturer. The manufacturers, grateful for the information, make quick adjustments in production.

It may have been noted that the period when the potency test was being developed and vaccines were for the first time being tested for potency, was the same period when an increase in the decline of both death and case rates started. The coincidence suggests to one that general improvement in the potency of vaccines was a contributory factor to the declines. Other possible factors will be discussed later.

The adoption of a standard of potency in 1949 did not solve all the problems. All lots of vaccine had a certain amount of mouse protective activity but there was considerable variation in the potency of the doses of vaccine recommended for human immunization. From the beginning of the use of all kinds of bacterial vaccines, the human dosage has been measured in numbers of bacteria. For pertussis vaccines, containing no mineral adjuvant such as alum, the numbers reported, to be effective, have ranged all the way from 22,000 million to 140,000 million. In 1949, the recommended total human immunizing dose of the different licensed pertussis vaccine products, without adjuvant, ranged from 60,000 million to 100,000 million bacteria. This variation introduced about a twofold chance for a difference in potency but the greatest chance for variation was in the bacteria themselves. The bacteria in some lots were four times more potent than in others. In the mouse test, the potency of the bacteria was determined by comparing the number of bacteria in the respective doses of the vaccine under test and the reference vaccine that protected 50 percent of the mice. Potency was satisfactory if the number of bacteria in the ED_{50} of the vaccine did not exceed the number in the ED_{50} of the reference.

Although aware of variations, we did not know how much of the mouse measured potency was required to protect children. If fact we did not know if there was any relation between mouse protection and human protection. Previous experiences with other products evaluated by animal tests, however, gave us confidence that a vaccine that had mouse protective activity would be better for children that one that had none. A field trial was planned but the Korean War interfered with obtaining federal support. Pertussis was not a war problem. In 1951 the Medical Research Council of England published a report of the first results obtained in a field study that was in progress. Protection afforded children was not correlated with the number of bacteria in the total dose administered. Two lots given in a total dose of 60,000 million afforded significantly better protection than others that were given in doses ranging from 90,000 million to 112,000 million.

With the best information available to us in 1952, a value of 12 units was assigned to that amount of the U. S. reference vaccine estimated to be sufficient to afford significant protection to a child. The number for the unit value was arbitrarily selected because it was divisible by 2, 3, and 4, the number of single doses in the total doses, then being recommended. The reference was designated as the U. S. Standard Pertussis Vaccine. It was freeze-dried and is used now only for standardization of working standard lots or for special comparative studies.

After a value was assigned to the reference we were able to determine the extent of potency variations that had been occurring in the lots of vaccines submitted to the National Institutes of Health for release. Data on 878 lots submitted during a 27 months' period were analyzed (Pittman, 1954). It was found that products containing the same number of bacteria showed as much as a fourfold difference in potency per total human dose. Some products containing high counts were less potent than others containing lower counts. There was an over all difference of 6.2-fold in the average value of lots of different manufacturers' products.

Since August 1953 it has been required that each lot of vaccine contains 12 units per recommended total human immunizing dose. This is one of the few products for which an upper as well as a lower limit has been placed on the amount of the protective activity per dose. With most other products, potency may exceed but in no case may it fall short of the minimum required. The upper limit for pertussis vaccine was adopted with the hopes of reducing untoward reactions such as fever, malaise, vomiting, local soreness, and the rare fatal encephalopathy. It was considered that an amount in excess of that needed to afford protection should not be used. Since this date, the number of complaints, received at the Division of Biologics Standards, of untoward reactions have been significantly reduced. No death has been reported to us. Eleven had been reported from 1944 through 1952, four of which occurred in 1952. No doubt, there have been other influential factors, one of which is that the manufacturer's accompanying circular of each product containing pertussis vaccine, now gives contraindications for injections.

Two papers, recently published, suggest that the mouse potency value assigned to 12 units is near the effective dose. One (Felton and Verwey, 1955) was the first report of a field trial in which the laboratory value of the vaccine was given. The two lots employed contained approximately 15 units per total human dose. Significant protection against pertussis was obtained. The other paper (Jaffe, 1955) suggests that low unit vaccines may not be effective. Dr. Jaffe, of the Department of Health of the District of Columbia, observed, from February 1, 1953, to March 15, 1954, 100 culturally proven cases of pertussis in two child-health clinics. Sixty-four of the cases had had completed courses of vaccination and 16 more had had at least one inoculation. Through the cooperation of the manufacturers, we were able to evaluate the potency of 90 per cent of the containers of vaccine from which it was estimated that these children would have been vaccinated. Only 41 per cent of the containers had 12 units per total dose, 34.5 per cent had values as low as 5.5 to 6.5 units, while the remaining 25 per cent had values ranging from 7.3 to 9.6 units. The lots had been released under the first minimum requirements.

Lest one gain the impression that the potency evaluation of pertussis vaccine is simple, it should be mentioned that the test as designed has a 1 in 20 probable error of about log 0.3. If each part of the test is carefully standardized, however, reproducible results are usually obtained. For an exact evaluation, we find that three tests are usually adequate. For routine work we seldom have to do more than one test to obtain satisfactory agreement with the test submitted by the manufacturer. Because of the probable variation in the test, limits of 8 and 36 units were set for an acceptable lot. The lower limit may be too low. If the protocol of a lot submitted, however, gives a borderline value, low or high, the lot is tested in our laboratory. A lot with a true high value will seldom pass the freedom-from-toxicity test. Frequent check is made irrespective of the manufacturer's results, so that more than 50 percent of all lots submitted are tested. All inspection samples are tested. An example of the potency evaluation of two lots of vaccine is given in Table 3.

Biologic products standards cannot be defined or measured with the same degree of accuracy as are physical standards. The products are organic chemicals largely of unknown composition. They are subject to change by the methods of preparation, the preservatives, and the time of storage. Further the measurement of activity is generally determined by the response of a biological system, usually animals, which may show wide variations in response.

TABLE 3.—POTENCY EVALUATION OF TWO LOTS OF PERTUSSIS VACCINE

Vaccine Number	Dose per mouse ml.	S/n	ED_{50}	Potency		
				Multiple of Standard per ml.	Units	
					1.0 ml.	Total dose
						1.5 ml.
X_1	.03125	14/16	.00625 ml.	.79	9.9	14.8
	.00625	8/16	± log 0.125			
	.00125	2/16	log 1.39* ± 0.61			
						3.0 ml.
X_2	.0625	15/16	.00722 ml.	.68	8.5	25.5
	.0125	11/16	± log 0.125			
	.0025	3/16	log 1.53* ± 0.67			
Standard	.02	15/16	.00493 ml.		12.5	
	.004	5/16	± log 0.118			
	.0008	2/16	log 1.64* ± 0.70			

* Slope of the curve.

Besides being of value in determining the potency of a finished pertussis vaccine, the test has been very valuable for selecting ways of improving the vaccine, for example: Selection of strains of bacteria with high antigenic properties, the development of simpler or better media, determination of the effect of preservatives and detoxifying agents on potency, and the purification of antigens.

The production of pertussis vaccine is complicated. The organism is quite exacting in nutritional requirements and it produces a thermolabile toxin. It has, as do all gram negative bacteria, an endotoxin. Very little is known about nutritional requirements in relation to protective antigens. One of the biggest problems is detoxification without affecting antigenicity. Physical or chemical agents that hasten detoxification may injure the protective antigen. This may be detected at once or only after 6 months or more in storage. An understanding of the enzyme activity of the bacterium might be a key to the solution of some of the problems.

In the United States, pertussis vaccine is frequently combined with diphtheria and tetanus toxoids and about 70 percent of all lots released contain a mineral adjuvant. Adjuvants are used to enhance the antibody response to a given amount of antigen. Recently a problem of toxicity caused by a mineral adjuvant was encountered. Studies with diphtheria toxoid have indicated that aluminum phosphate was better than alum. When the recommended amounts of the adjuvant were used in products containing pertussis vaccine, a lot frequently failed to pass the pertussis vaccine freedom-from-toxicity test. Adjustments were made by reducing the amount of adjuvant and no doubt without loss of the potentiating effect on the toxoids as pertussis vaccine itself acts as an adjuvant.

We look forward to the day when purified preparations of the protective antigen will be available. Two partially purified preparations have been prepared in different laboratories (Pennell and Thiele, 1951, and

Pillemer, et al, 1954). The antigens differ markedly in one respect. One induces higher agglutinin antibodies than the parent whole-cell vaccine (Felton and Verwey, 1955). The other one contains practically no agglutinogen (Evans and Perkins, 1955). Both are capable of protecting mice against intracerebral infection. The former was found to be effective in protecting children against pertussis. We await the results to be obtained from the latter antigen which is on field trial in England.

DISCUSSION

It was suggested earlier that the establishment of the U. S. Standard of Potency for pertussis vaccine was an influential factor in the reduction of both death and case rates of pertussis. It has ever been as Magill, 1955, recently stated that "the physician or the medicine man of each age has attributed the control and cure of each disease during his own particular era to the therapeutic procedures in vogue". Today there is a tendency for immunologists to attribute the decline of infectious diseases to an induced immunological state. Let us look at history.

Many infectious diseases have shown cycles of high incidence followed by low incidence. Between 1850 and 1860 diphtheria became much more prevalent and malignant, the change was so marked that the physicians of the day often spoke of it as a "new" disease. Mortality rose to extraordinary heights and remained there for about 30 years, around 1885, then a consistent decline began. This was before the introduction of antitoxin. The rate of decline was increased after toxoid vaccination came into general use.

The cycles of pandemic influenza strikingly illustrate changes in incidence. Further, with this disease is illustrated the shift from endemic to epidemic and from epidemic to endemic periods between pandemics. It has been shown that the antigenic pattern of the virus may change from one epidemic to another epidemic.

In figure 1 we observed the decline in the death rate of measles against which an immunizing antigen had not been used. In the same figure there was shown the decline in pertussis fatality rate before pertussis vaccine was in general use. In 1943, Dauer suggested that if the mortality continues to decline at the same rate as it did from 1925 to 1940, it will be difficult to show statistically that a prophylactic procedure has any effect in reducing mortality from pertussis.

In the 15 years since his analysis, pertussis vaccine has come into general use, and in the last ten years there has been an over-all improvement in the potency of vaccine. During this time there has been an accelerated decline in mortality and for the first time a decline in morbidity.

Although the use of improved vaccine no doubt has had some influence on the decline, it appears that we may have been observing the downward trend of a cycle. In the older writings, some dating back to Hippocrates, there appear descriptions of some diseases in such detail that certain of the common communicable diseases can be readily identified. The first known written description of whooping cough was by Ballonius in 1578. It is difficult to understand how a disease characterized by the paroxysmal cough that does not occur in any other infectious disease and with the high fatality rate observed at the turn of the century, could have escaped description. Could it be that pertussis was once a mild disease and that we are approaching a period when it will be again a mild and less frequently recognized disease?

We have observed that there was a significant decline in deaths long before there was a decline in the number of reported cases. This could be interpreted as a reduction in the severity of the infection. Dr. Jaffe's observations of 100 bacteriologically proven cases of pertussis is compatible with a change in the severity of the infection. Most of her cases were mild. None were hospitalized and all recovered. The only significant complication was one middle ear infection. With an apparent change in the severity of the disease, no doubt many cases are not being reported. If so, this would have contributed to the decline in reported cases. As of January this year, pertussis has been omitted from the weekly telegraphic reports of the States.

Whatever is the cause for the decline in pertussis, be it a change in the antigen of the

microorganism, a change in the immunological condition of the host, the use of antipertussis serum or antibiotics, neither of which effect an immediate relief of symptoms but may arrest progression and shorten the duration of the disease, or other factors, it is suggestive that a state of equilibrium is being established between the microbe and the host. Nevertheless, we should not cease trying to prepare the most effective prophylactic vaccine free from undesirable activity or stop fundamental research both on a bacterium which has so many individual characteristics and which induces a disease with many anomalies (Gordon and Hood, 1951) and on the mechanism whereby in a local disease such marked neurological complications may occur.

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In every explanation of natural phenomena, we are compelled to leave the sphere of sense perceptions, and to pass to things which are not the objects of sense, and are defined only by abstract conceptions.—HELMHOLTZ.

PHYSICS.—*Cosmological theories—ancient and modern.*[1]

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In the month of December 1955 radio astronomers of the Naval Research Laboratory had their high-resolution radio telescope focused on Cygnus A. They were constructing a power spectrum across the region of the hydrogen spectrum line at 21 cm wavelength. They were looking for a dip or absorption line in the spectrum, which would occur if the energy received were passing through or were eclipsed from a hydrogen cloud. Furthermore, they were looking specifically for such an absorption line at a wavelength longer than 21 cm, which would occur if the hydrogen cloud were associated with Cygnus A source and therefore moving as Cygnus A is believed to move relative to the earth. On December 27 they found exactly what they were looking for. And when, by careful measurement and calculation, they derived the apparent velocity of recession from the earth, they found it to be 16,700 km per second. This agrees with a similar conclusion based on optical red-shift data to within less than 1 percent.

Let us look for a moment at what is back of this remarkable observation. It can hardly be called a new scientific discovery, since it was so confidently expected on the basis of all our other knowledge concerning our universe. Nor can those who conducted the experiment be said to be superior to other radio astronomers because they made the observation, since the experiment was made possible by the fine precision instruments made available to them. What were these instruments, and how did they come into being?

The instruments consisted primarily of a large, precision-built radio antenna, an accurately controlled servo-driven antenna mount, a high-gain, highly stable, selective, tunable, radio-frequency receiver, and a precision frequency control. These are all products of a relatively new field of endeavor, radio research and engineering. A considerable portion was built after the war, based in part on the results of wartime research. As a radio astronomy facility it has been in existence less than 10 years.

May we now turn to the much larger subject, our other knowledge concerning our universe which prompted the radio astronomers to look in the direction of Cygnus A, for the hydrogen radio frequency absorption line, at an augmented wavelength? The hydrogen absorption line at radio frequency was first observed in 1954 by radio astronomers of the Naval Research Laboratory using the apparatus just mentioned. Its existence was expected because the hydrogen radio-frequency emission line had been observed, first at Harvard University in March 1951, and subsequently in Holland and Australia.

Now we ask the question: What led to the hydrogen emission line observation at Harvard in 1951? The answer is simple. The radio astronomers there were looking specifically for that signal. They were looking for it because they had the precision radio astronomy instrumentation capable of detecting it if it were there, and the Dutch astronomer H. C. van de Hulst had predicted seven years earlier, in 1944, that such line should exist. In fact, van de Hulst was at Harvard at the time the observation was made, and I should not be surprised if he actually stimulated the Harvard astronomers to conduct the experiment, if, indeed, he did not himself participate in it.

But now we face another question: What led van de Hulst to make such a prediction? A commonly accepted model of the hydrogen atom is a proton nucleus about which orbits one electron. Both proton and electron spin on their own axes. The two spin axes of any one hydrogen atom at any particular time may be either parallel or antiparallel, i.e., while the proton spins in one direction of rotation, the electron may spin either in the same or in the opposite direction of rotation. Since both proton and electron are charged particles, their spin axes are also their magnetic axes. When their magnetic axes are

[1] A lecture given before the Philosophical Society of Washington on January 27, 1956.

parallel they repell each other and the electron orbit is slightly enlarged, resulting in a slightly increased potential energy state. When their magnetic axes are antiparallel, they attract each other, resulting in a slightly decreased potential energy state. The energy difference between these two states, when the electron is in the base orbit, was calculated by van de Hulst and divided by Planck's constant to yield the frequency value 1,420,405,000 cycles per second, or approximately 21 cm wavelength. To radiate energy at this wavelength, the hydrogen atoms must receive energy from some external source, in sufficiently large units to raise the atomic potential energy from the lower to the higher of these two levels, or, in terms of our model, to reverse the direction of the spin axis of the electron relative to the proton. Then, since this is not a stable state of affairs for the atom, the spin axis will flip back to its normal state, and in the process, radiate one quantum of energy at a wavelength of 21 cm. Van de Hulst then called attention to the possibility of this mechanism.

To trace to their origins the concepts on which van de Hulst made his suggestion would be far too involved for present treatment. It would lead us back through atomic theory, electricity and magnetism, optics, classical and statistical mechanics, mathematics of almost every branch, and a large array of scientific instruments. We shall simply leave it at that.

Thus far we have dealt only with one of the three questions we raised: Why were the radio astronomers last December looking for a hydrogen absorption line in the region of 21 cm wavelength? Let us now turn to a second one of the three questions: Why were they looking for an elongated wavelength?

The answer again is simple. The visible light we receive from distant galaxies is similarly elongated in wavelength. Let us explore the basis of this simple but somewhat categorical answer. We just considered a hydrogen atom to be a proton nucleus with an electron circling around it in one of several possible orbits. The atoms of all the other elements are likewise considered as consisting of nuclei in which protons predominate, and about which a number of electrons circle in a number of possible orbits. And as in the hydrogen atom, so in all atoms, energy may be absorbed in moving one or more electrons from their normal orbits to orbits of higher potential energy, whereupon the normal state will restore itself with the release of the absorbed energy in the form of electromagnetic radiation, at a frequency which is directly proportional to the energy difference between the two levels involved. The atoms of each element have a pattern of electron orbits which differs from the corresponding patterns of all other elements. When an element is excited so as to produce these energy transfers, the pattern of frequencies radiated is a "fingerprint" of that element. The pattern of frequencies is obtained by passing light from the excited element through a spectrometer, which separates the incident light into its frequency components and indicates the intensity of each frequency present. When light from distant nebulae is caught in an astronomical telescope and focused on a spectrometer, the spectra or "fingerprints" of known elements are found, but the wavelengths of all the spectrum lines from most of the nebulae are elongated. The light, in other words, is shifted toward the red end of the spectrum. Scientists have taxed their imaginations to explain this red-shift, and the only suggestion that satisfies is that it is produced by the Doppler effect of relative motion. In other words, the galaxies are moving away from us.

Now we could probe much more deeply into the origins of all the concepts touched upon but lightly here, of how J. J. Thomson discovered the electron, of how Ernest Rutherford discovered the hydrogen nucleus and postulated the basic structure of our present atomic model, of how R. A. Millikan measured the charge of the electron, of how Max Planck developed the quantum theory and arrived at the constant of proportionality between energy and frequency, of how Nils Bohr applied this theory to Rutherford's atomic model to give us our present atomic theory, of how the spectrometer works and what led to its invention, and of all the science and engineering that is associated with the invention and operation of astronomical telescopes, and of photosensitive

emulsions, and the optics and chemistry and mechanics of their use in both telescope and spectrometer. Many books have been written on these subjects, and to most of us they have become such an "old story" that we tend to take them for granted and forget the extent of our indebtedness to a long succession of technological developments in instrumentation for scientific observation which provided the means for all the scientific discoveries mentioned here, and from which we have inherited a galaxy of precision instruments that would completely mystify the ancients. But in these instruments, their skillful use, and the careful and clever interpretation of the data they provide, lies the answer to the question: "Why were the radio astronomers looking for an elongation of the hydrogen line wavelength that is normally 21 cm?"

We come now to the third question: Why were they using Cygnus A as a source for making this measurement? The answer to this leads into cosmological theory, the foundation for which has been partly developed in answering the other two questions. Cygnus A was unknown to astronomy until discovered by radio in 1948. It was then thought to be a part of our own galaxy, although the possibility of its being extra galactic was pointed out. When the 200-inch optical telescope was turned in its direction to investigate, Cygnus A was found to be two galaxies in collision, with the red-shift in its spectrum indicating a distance of tens of millions of light years. Needless to say, this discovery created considerable excitement in astronomy, particularly in the field of radio astronomy, and it became a matter of great interest to determine whether a frequency shift in the radio frequency hydrogen line might be found to correspond to the optical red-shift.

We have shown how distant galaxies appear to be flying away from us as sparks from the blacksmith's anvil. We have stated that more remote ones appear to be receding faster, but we have said nothing about why we think the greater apparent velocities are associated with greater distances. How do we measure these distances anyway? Let us start with the shorter distances, say a few tens or hundreds of light years. First, we must have a reference distance, or "measuring stick." This is the diameter of the earth's orbit about the sun. By recording the difference in position of a nearby star relative to the remote stellar background between two measurements made six months and some 186 million miles apart, the distance to that star can be calculated. This method of measurement gives good results out to about 100 light years, less accurate but usable results to several hundred light years, and can reach about 5,000 stars. For greater distances a particular type of star called a Cepheid variable is used. A Cepheid variable is a star that pulsates in brightness with a period of from a few hours to many days. The absolute brightness of such a star has been found to be directly proportional to its period of pulsation. The constant of proportionality was determined by observing the periods and apparent brightnesses of Cepheid variables in our vicinity, where distances could be determined by triangulation and by star motions. Actually there are two types of Cepheid variables, blue giants and red giants. For a given period of pulsation, the red giants are about 1½ magnitudes brighter than the blue giants. This "double standard" was not fully appreciated and accepted until only two or three years ago. It was the discovery that the Cepheid variables near us, used for calibration, were all blue, while the remote ones used for distance measurement were red giants, making the measuring stick in use more than twice as long as it was thought to be, that doubled all remote astronomical distance and time estimates only just recently. Gamow gives the factor as 2.8.

Cepheid variables are available for determining astronomical distances as far as they may be resolved as individual stars and their variations measured with telescope and photomultiplier. With the 200-inch telescope, this is about 20-million light years. Now we have one measuring technique that takes us out to 100 light years, and another that takes us out to 20-million light years. Yet another is needed for the many galaxies visible to the telescope but beyond the range at which individual stars may be resolved.

We have in fact two such measuring techniques. The first is apparent brightness.

Nearly all galaxies seem to have the same absolute total brightness within a factor of approximately two from the mean value. By assuming mean absolute brightness and measuring apparent brightness, distance may be calculated hopefully within a factor of two. The other technique uses the spectrum red-shift. Since those galaxies whose distances may be measured by Cepheid variables which they contain show a red-shift proportional to their distance from us, it is not unreasonable to assume that this law of red-shift holds for greater distances, at least within certain limits. Red-shifts have been measured out to a shift of a little over 20 percent, corresponding to a recession velocity of 38,000 miles a second, and a distance of about a billion light years if the law holds that far. For greater distances, apparent brightness remains the only clue to distance. On that basis the present maximum reach of the 200-inch telescope is approximately 2 billion light years. It must be remembered, however, that at that distance we see galaxies as they appeared 2 billion years ago—time enough for significant changes to occur in their brightness. Also, if the law of red-shift holds at that distance, indicated recession velocities would be on the order of one half the velocity of light, and relativistic effects may be present. No one has yet been sufficiently daring to suggest what influence these effects might have on apparent brightness, if, indeed the elementary spectra or "fingerprints" of the atoms themselves may not be altered.

All this while we have been explaining why radio astronomers made a certain observation last December. In so doing, we have reviewed enough elementary astronomical observations to discuss modern theories pertaining to the nature and origin of the universe. We start with the assumption that the laws of nature are uniform and consistent throughout all space and all time. If remote bodies are receding from us they must at some time in the past have been much closer to us and closer together. In fact, if their relative velocities are proportional to their relative distances, extrapolation backward in time should yield a definite time in the past when all the matter of the universe was tightly packed in one place, from which it is exploding like one great super atomic bomb. This extrapolation places the time of explosion about 4 to 5 billion years ago. This view is held by a number of scientists, one of whom, George Gamow, has given us a fairly detailed account of how it might have happened. We will not attempt to probe into the scientific bases of this account, as Gamow and others have done, but rather attempt to describe a probable sequence of events.

In the original tightly compressed state, which is the earliest state science can know anything about, the temperature would be too high for matter to exist at all, and all the sum total of the matter and energy of the universe would be in the form of radiant energy. Thus the earliest phenomenon that can be postulated with any scientific foundation is a blinding flash of light intense beyond the capabilities of human imagination to conceive. On expanding from its point of origin, it would cool, at first with extreme rapidity. Five minutes after the first generation of this energy, the temperature would have cooled down to a billion degrees. At this temperature, protons, neutrons, and electrons could exist, but not atoms. In the succeeding 25 minutes all the chemical elements would be formed, for at the end of the first half hour the temperature would have dropped too low for nuclear reactions to take place. Calculations of the relative abundance of the elements that would result from such a process agrees remarkably well with what is found to exist today. Also, the state of decay of radioactive elements indicates that they were originally formed about 5 billion years ago.

Today the mass density of matter in the universe far exceeds the mass density of radiation. If, as has been postulated, the universe started out as all radiation, there must have been a time when the total mass density was equally divided between radiation and matter. On this subject let me quote from Gamow. "Computing the mass densities of radiation and of matter at various epochs, we can find the date of the great event when matter took over from radiation, i.e., surpassed it in mass density. The date was about the year 250,000,000 A.B. (After the Beginning). The temperature of space

was then about 170 degrees absolute, and the density both of radiation and of matter was comparable with the present density of interstellar gas. The Universe, in short, was dark and cool." This statement appeared in the March 1954 *Scientific American*.

The gradual transition from radiation to matter has been likened to slow precipitation of a solid from solution. As matter gained the ascendency over radiation, it began to react to the forces of turbulence and gravitational attraction, and formed into great clouds of gas. In time these clouds contracted by gravitational attraction to form the beginnings of galaxies. But turbulence, consisting of eddies within eddies ad infinitum, produced many secondary and tertiary centers of contraction, so that the clouds of gas "coagulated" into small "lumps," which in turn fell into larger ones, until whole hierarchies of suns and planets and satellites were formed. The pressures created by gravitational attraction produced local heating, the larger accumulations becoming quite hot. The maximum temperature thus produced in any star depends on the mass of the star. To reach surface incandescence requires a mass about 2,000 times that of the earth. Some galaxies are imbedded in gas and still growing. They are blue in color and spiral in form. Others have used up all their gas, and are cooling off. They are red in color and globular in form.

We have given in brief outline one of the two principal modern theories of cosmology. It is based on observed physical phenomena and analogous mathematical models. Whether the present hyperbolic expansion was matched by a corresponding hyperbolic contraction before the point of maximum concentration, or whether the primeval flash of light was an original creation must forever remain beyond the reach of science to decide. The fact remains that this theory provides a satisfactory scientific explanation for an amazingly wide variety of observed phenomena. Furthermore, it yields a value for the age of the universe which is remarkably consistent with no less than 12 other and mutually independent indications of that age. Space does not permit exploration of all these other methods, for we have other theories to consider.

One of these other theories is also a modern one, having originated only a few years ago, before discovery of the two populations of Cepheid variables. Two objections were raised to the "explosive" theory. One was the discrepancy in the age of the universe dictated by the theory as it then appeared. The other was that it tended to support the Biblical account of creation, a situation some found to be unpalatable. So in 1948, Bondi and Gold, as well as Hoyle, advanced a "steady-state" theory, in which matter is created continuously, one proton at a time, throughout the universe, to replenish the fleeing supply. A mathematical model has been constructed on this concept and made to fit present observations. It must be recognized, however, that the steady state theory is pure conjecture and has not a single experiment or physical observation to support it that does not also support the explosion theory. Furthermore, instead of disagreeing by a factor of 2 or 3 with other methods in indicating the age of the universe, it makes the universe eternal. Since its introduction, however, the correction to the astronomical measuring rod has brought the explosive theory of Eddington, Lemaître, Gamow, and von Weizsacker into complete harmony with other methods of age determination. That leaves only one argument for the continuous theory over the explosive theory: namely, it appears to contradict the Biblical statement of creation. Even in this matter however, it does not fully escape criticism. If matter has to be created out of nothing ultimately, is it any greater miracle to create one neutron at a time, than to do it all at once? Certainly there is difference in degree, or in rate of generation if you please, but where is the difference in *kind*? The whole argument reminds me of the heart specialist who was called on to treat a patient with a broken leg. The doctor treated him for a weak heart because he didn't like to mess with broken bones. I do not mean to ridicule any man who is honestly searching for truth. I am only pointing out that the temptation to interpret observations so as to make them support conclusions

already reached is as dangerous as it is difficult to avoid. Let me hasten to add that I make no pretense of immunity from the hazard.

Now let us return to our explosive model and speculate on still greater distances. If the law of red-shifts does in fact arise from the flight of the galaxies, then at the distance of 5 billion light years the velocity of recession will equal the velocity of light. Any galaxies beyond this limit would have to be fleeing from us at an even greater velocity, and their light would never reach us at all. Thus we have an astronomical horizon beyond which we can never see with any possible kind of instrument.

We have no reason to assume, however, that the earth is located at a point of origin of galactic motion. Were we far removed from such a point, all galaxies would still appear to be receding from us. Assuming such an origin to exist, let us transfer our velocity reference point from the earth to the locale in space where it all started. From that point the universe moves away in all directions with all possible velocities. If the highest velocity of expansion is much less than the velocity of light, the outer boundary of the universe at any one time would be nebulous and diffuse. If, on the other hand, the original energy was such as to produce velocities greater than the velocity of light in the absence of relativistic effects, the presence of those relativistic effects would restrain the velocities to less than the speed of light, and the momentum would be maintained by the corresponding relativistic increase in mass. The result might be a "piling up" of galaxies at the outer boundary of the expanding universe, moving outward as a spherical shell or sort of "shock wave" of light at very nearly, if not exactly, the velocity of light. Such a shell would hardly be visible at the point of origin, but part of it might be visible at points far out from the origin. If this be the case, then, if the earth is sufficiently far removed from the primeval origin, and if telescopes are made capable of seeing far enough, it should be possible to find an increasing density of galaxies in the direction of least distance to the shell or outer boundary. The mathematical model which seems consistent with this concept is described under the title of "curved space," in which the radius of curvature in our universe is said to be 5 billion light years.

Let us now recapitulate the model we have been describing in terms of the origin and evolution of the earth. We start with the phenomenal burst of radiant energy, the solvent for all matter, and call it the birth of our universe. In the first 30 minutes we see all the elementary particles formed and organized into atomic nucleii. Then nothing but cooling and expanding as matter continues to precipitate out of radiation, until all is dark and cold. Then, slowly at first, a great cloud begins to form out of the turbulence, and separate itself from other similarly forming clouds as they all shrink into more dense masses of gas. Then coagulating lumps of liquids and solids begin to form, and little lumps fall into bigger lumps, until certain large ones begin to draw to themselves everything near them. And since the whole cloud was rotating as a part of the general turbulence, all the bodies were also rotating as they formed, the speed of rotation of each body increasing as matter was drawn together in smaller volumes. And as the large bodies grew larger, pressures at their centers increased, with corresponding increase in temperature, until the larger ones became incandescent, shining one by one, all through the galactic system. One of these stars was our sun, and when it "lit up" it illuminated a host of planets with their satellites. One of these planets was the earth, which had coagulated from a little whirlpool of gas within the greater "whirlpool" of the solar system, which in turn was part of the turbulence of the whole galaxy. When the earth reached its maximum temperature, it was too hot to retain water, so all the water of the oceans and the moisture of the soil existed as a dense shroud of steam completely enveloping the earth and continuous right down to the earth's surface. And as the earth cooled, the steam condensed into pools of hot water on the surface. Eventually the moisture in the air dropped below the saturation point, and the fog began to rise, leaving a clear separation between the water surface and the cloud blanket overhead,

much as we see it occasionally now. Then the wet land began to become dry by evaporation, and conditions were favorable for the appearance of vegetation. When vegetation appeared, it sustained itself by reproduction, according to laws of heredity that have been the subject of much study since Mendel's time. The mechanism seems to be that each kind of plant has its seed within itself and reproduces after its kind.

As moisture continued to condense and fall as rain to the earth, the cloud blanket became thinner and ultimately broke up. Then for the first time the sun, moon, and stars were visible on the surface of the earth, and available for telling the time of day and the seasons of the year.

By now we should have a fairly good bird's-eye view of what our universe is and how it got to be that way, according to the most probable modern scientific speculation. Let us turn now to some other cosmologies, pausing first to contrast the scientific atmospheres of past and present. Science to us is a partnership between philosophy and technology. We more familiarly refer to these two aspects today as theory and experiment. This partnership was first seriously joined by Sir Isaac Newton and has been growing with accelerating fruitfulness ever since, particularly as mathematics became increasingly a major tool of both. Before Newton's time, fruitful interactions between the two were rare. When we probe farther back to the Greeks and the Egyptians, the Hebrews and the Babylonians, it was as if neither was aware of the existence of the other, unless to spurn it as a degrading influence on mankind. Even had the thought occurred to form such a partnership, the crude technology of ancient times was a poor match for the philosophical conviction that all natural phenomena were direct actions by conscious gods, whose behavior was as capricious as that of men. It is important that we recognize this when dealing with ancient cosmologies, and maintain a sympathetic attitude as we attempt to place outselves in the position of ancient philosophers.

There are many cosmologies among the mythologies of antiquity. We can not discuss them all, but we will examine two of them. First we will review what has been called the Babylonian Genesis, the *Enuma Elish* ("When above"):

> When above the heaven had not (yet) been named,
> (And) below the earth had not (yet) been called by a name;
> (When) only Apsu privemal, their begetter, (existed),
> (And) mother Ti'amat, who gave birth to them all;
> (When) their waters (still) mingled together,
> (And) no dry land had been formed (and) not (even) a marsh could be seen;
> When none of the gods had been brought into being,
> (When) they had not (yet) been called by (their) names, and
> (their) destinies had not (yet) been fixed;
> Then were the gods created in the midst of them.

The created gods were the sons and grandsons, daughters and granddaughters of Apsu and Ti'amat. But the children always became greater than their parents, and they also became mischievous and annoying, as younger generations sometimes do, until the old grandparents, Apsu and Ti'amat, could not rest. Finally Apsu decided to put an end to the annoyance:

> Apsu opened his mouth
> And said to Ti'amat, the holy (?) one:
> "Their way is annoying to me,
> By day I cannot rest, by night I cannot sleep;
> I will destroy (them) and put an end to their way,
> That silence be established, and then let us rest!"
> When Ti'amat heard this,
> She was wroth and cried out to her husband;
> She cried out and raged furiously, she alone.
> (For) the malice (of Apsu) disturbed her heart.
> "Why should we destroy that which we have brought forth?
> Their way is indeed very annoying, but let us take it good humoredly!"

But Apsu would not be dissuaded, and he plotted to kill his children. But the plot leaked out, and he himself was killed by his own offspring. In ensuing conflicts, Ti'amat was slain by Marduke, who drained out her

blood and let the wind carry it away. He then split her body in two, made the vault of heaven from one half, and from the other half, made the earth. Gods who had supported Ti'amat were enslaved. When they complained of their slavery, the kind-hearted Marduke took their leader's blood and mixed it with clay to make man. Then he assigned to men the task of serving the gods, and set the captive gods free.

This whole account is recorded in cuneiform writing on clay tablets. It consists of seven tablets, totaling over a thousand lines in all, of which approximately 800 or 900 have been recorded and translated. It is representative of the general character of most mythological cosmologies. As one might expect, it bears no real similarity to our own modern cosmology. Under the circumstances, this is not surprising.

Now permit me to review just one more ancient cosmology. This one I will give in an unpublished translation, since the published translations are old, words change their meaning from generation to generation, and recent findings of archeology and philology have added to our concepts of what the originals really meant. This is the cosmology of the ancient Hebrews, and in one form or another may have been nearly contemporaneous with the Babylonian. I paraphrase freely, in the attempt to recapture the original thought as determined by the work of modern scholars, making liberal use of the terminology of modern cosmology.

In beginning, Gods created the heaven and the earth. And the earth was without form, and nebulous, and darkness reigned throughout all space. And the Spirit of God was brooding upon the face of the waters. And God said "Let there be light," and light appeared. And God saw the light, that it was good. And God divided time into periods of light and darkness. And God called the time of light Day, and the time of darkness he called Night. And this completed the first epoch of the creation of the earth.

And God said "Let there be an expanse of clear space in the midst of the waters, and let it divide one part from another." And God made the expanse of clear space above the earth, and divided the waters which were under the clear space from the waters which were above the clear space. And God called the expanse of clear space Heaven. And this ended the second epoch of the creation of the earth.

And God said, "Let all water under the heaven be gathered together into one bed, and let the dry land appear." And it was so. And God said, "Let the earth bring forth grass, the herb yielding seed, and the tree yielding fruit; whose seed was in itself, after his kind." And God saw that it was good. And this ended the third epoch of the creation of the earth.

And God said, "Let there be lights in the expanse of heaven to divide day from night, and for indication of the seasons, days, and years, and let them shine in heaven to give their light on the earth." And God caused to shine on the earth two great lights, the greater for daytime, the lesser for night time. The stars also he made to shine on the earth from the expanse of heaven.

There is more in this Hebrew cosmology with which we will not concern ourselves at this time, since it deals with a quite different realm of science than we are considering. I should point out, however, that the account of the appearance of lights for indicating times and seasons does not make reference to original creation of the lights at this time, but rather the making available of the lights to the earth, such as would occur by a clearing away of the clouds of moisture around the earth.

I think this very cursory review suggests a parallelism between the Hebrew cosmogony and our own. This parallelism is rendered even more remarkable by its striking contrast to the corresponding Babylonian version. It is true that some students of ancient records have attempted to show an extensive parallelism between the Babylonian and the Hebrew cosmogonies. Close scrutiny, however, shows the points of similarity to be purely superficial, and of far less consequence than the overwhelming weight of the contrasts. On the other hand, the amazing consistency between the concepts of Hebrew cosmogony and our own poses a question which science has not answered: How did those ancient Hebrews without aid of telescope, spectrometer, electronics, atomic theory, mathematics, and all the other components of the foundations of modern cosmology, come into possession of the comprehension of prehistoric nature exhibited in their cosmogony?

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Let us remember, please, that the search for the constitution of the world is one of the greatest and noblest problems presented by nature.—G. GALILEI.

PHARMACOLOGY.—*Some metabolic patterns observed after morphine administration in the rabbit.* LOUIS LEVY, Georgetown Medical School.[1] (Communicated by Theodore Koppanyi.)

Recent investigations concerning the mechanism of action of morphine on the enzymatic level (*1*, *2*) arrive at the conclusion that there is little correlation between the pharmacological action of morphine and its action on the enzyme systems studied. All attempts to describe the action of morphine on an enzymatic level have shown inconsistent patterns of enzyme inhibition, or require concentrations of morphine much higher than found to be effective in the intact animal (*3*).

A further review of the literature revealed that there have been very few studies recently on the chemical constituents of the blood in morphinized animals. During the past decade many new tools for chemical analysis of the body fluids have become available. A great advantage of these techniques and methods is their ability to identify small quantities of substances. Perhaps one of the simplest and most sensitive methods has been paper chromatography. This technique was used throughout this investigation for qualitative determination of blood constituents.

METHODS

Albino rabbits weighing 2 to 3 kgm were used throughout the experiments. All injections of morphine were given subcutaneously as the sulfate salt. Blood was taken from the marginal ear vein in all experiments. Before all sugar determinations, food was withheld for 18 hours. Preliminary experiments revealed that fasting was not necessary for reproducible results with blood amino acid nitrogen. Blood sugar was determined by the Somogyi-Nelson method (*4*), and blood amino-acid nitrogen was determined by the naphthoquinine method (*5*) after deproteinization as described by Hunter (*6*).

Ascending paper chromatography was used throughout the experiments. The deproteinized samples for sugar chromatography were dried at 50° C. and redissolved in absolute methanol. The samples were concentrated approximately twofold. The solvent system used was a mixture of n-butanol, pyridine, benzene and water (5:3:1:3). Aniline citrate was used to identify the aldoses, and orcinol, the ketoses.

Amino acids were identified, using 2-dimensional chromatography. The samples were deioned according to the method of Smith and Tuller (*7*) and then dried in a jet of air and concentrated tenfold. Eighty percent pyridine was the solvent used in one dimension and 70 percent ethanol in the other. A 0.2 percent ninhydrin solution in water—saturated butanol was used as the spray reagent to identify the amino acids. In identifying the individual amino acids, maps of known mixtures were used.

RESULTS AND DISCUSSION

The biochemical phase of morphine which has received the most attention has been the hyperglycemia observed in rats, rabbits, and dogs as well as certain other species after the administration of morphine. Because this was the most apparent biochemical lesion, and rabbits are particularly susceptible to this change, the first phase of this study concerned itself with the hyperglycemic response of rabbits to morphine.

Blood sugar determinations were made on eight fasting rabbits ½, 1, 2, and 4 hours after morphine. The hyperglycemia observed had been reported previously (*8*). This hyperglycemia disappeared after a week of daily injections, and even when the dose of morphine was elevated again, the rise in blood sugar was very slight.

After two weeks of daily injections of morphine sulfate, two of the rabbits were given nalorphine (10 mgm/kgm) daily instead of morphine at the time they were usually given the morphine. Two other rabbits were given daily saline placebos in place of the usual morphine. Blood sugar levels of these

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rabbits were determined daily as before. This treatment was kept up for one week; at the end of this time, a single injection of morphine sulfate (30 mgm/kgm) was again given to the rabbits.

During the week of nalorphine and placebo injections there was no significant rise in the blood sugar in either group of animals. Emerson and Phatak (*8*) as well as Phatak and David (*9*) in longer experimental periods claimed that when morphine administration was abruptly stopped in rabbits, there was a rise in the blood sugar. However, examination of these data reveals that the rise was very small. Nalorphine has been shown to precipitate withdrawal symptoms and if hyperglycemia is part of a withdrawal phenomenon in rabbits, it was not observed in this experiment. Therefore, if the return to normoglycemic response during continued morphine administration is indicative of addiction in the rabbit, withdrawal responses should have appeared in both the nalorphine animals and saline controls. Thus it appears that the tolerance to morphine hyperglycemia in rabbits is not synonymous with morphine addiction and can be considered a non-specific stress response.

Upon reinjection of morphine sulfate (30 mgm/kgm) both the nalorphine and placebo animals had quantitatively and qualitatively similar rises in blood sugar. The nalorphine animals had an average fasting blood sugar of 87 mgm/100 cc, and one hour after morphine it was at a maximum of 158 mgm/100 cc, a rise of 180 percent. The saline-treated animals had an average fasting blood sugar of 70 mgm/100 cc, and one hour after morphine it was at a maximum of 128 mgm/100 cc, a rise of 183 percent (Table 1). Thus it did not appear that the presence of nalorphine for one week conditioned the animal body to the presence of morphine with regard to the hyperglycemia. In acute experiments nalorphine has been shown to block the morphine hyperglycemia (*10*). In the experiments just described, the action of nalorphine had apparently worn off and it was no longer effective in blocking the rise in blood sugar.

The general methods for determining blood sugar depend on the reduction of some metallic ion by glucose. However, glucose is not the only substance in the blood which can reduce the usual copper reagents. Early reports (*11*, *12*) presented evidence of substances other than glucose present in the blood of morphinized animals which caused an apparent hyperglycemia.

TABLE 1.—RELATIVE BLOOD SUGAR LEVELS AFTER INJECTION OF MORPHINE, NALORPHINE, AND SALINE

Treatment mgm./kgm	*Percent rise above control* (1 hr. samples)
	Percent
Morphine sulfate—20, 1st day	130
Morphine sulfate—20, 4th day	14
Morphine sulfate—30, 1st day	35
Morphine sulfate—30, 4th day	17
Nalorphine—10, 1st day	10
Saline placebo, 1st day	26
Nalorphine—10, 6th day	4
Saline placebo, 6th day	0
Morphine sulfate—30, after nalorphine	80
Morphine sulfate—30, after saline	83

Paper chromatograms of the samples used in blood-sugar determination revealed only one spot when aniline citrate was the spray reagent. This reagent reacts with aldohexoses and aldopentoses. This spot was glucose and was obviously darker after the morphine injection than before, indicating that glucose was the reducing substance causing the hyperglycemia. Three different solvent systems were tried and eight different spray reagents were used and all revealed the same patterns.

The next experiment was designed to obtain some information concerning protein and/or amino acid metabolism. Kruger, Eddy, and Sumwalt state in their review (*13*); "The evidence is sufficient to indicate deep-seated disturbances in the nitrogen economy of the body." Although this was written 14 years ago, a review of the literature reveals no work has been done during the interim on the nitrogen economy of the body after morphine administration. With this in mind and also the possibility that some mechanism might be revealed, a study of the effect of morphine on the blood amino acids was undertaken.

Blood samples were obtained before and 1 hour after subcutaneous injections of 20 mgm/kgm of morphine sulfate on a different group of rabbits. The amino-acid nitrogen before injection was 10.1 mgm percent and

1 hour after morphine it was 9.1 percent. This 1 milligram difference was significant to the 0.1 percent level using the "t" test.

Paper chromatograms revealed at least 20 different identifiable amino acids. After morphine, these 20 amino acids were still present, although obviously in lower concentration. The chromatograms indicated that there was no qualitative change in amino-acid distribution after morphine.

The physiological significance and interpretation of changes in blood amino acid nitrogen is still in question, and the literature on the subject is very limited. However, a drop in amino-acid nitrogen has been shown to occur when epinephrine is given to animals (*14*). Since the rise in blood sugar after morphine has been shown to be caused by a release of epinephrine (*15*), the fall in blood amino acid nitrogen could also be attributed to this epinephrine rise.

SUMMARY

Through the use of chromatography, glucose was found to be the major reducing substance in the blood, causing the morphine hyperglycemia. Previously reported withdrawal hyperglycemia was not observed in this short-term experiment. Hyperglycemia was also believed to be a result of increased secretion of epinephrine. There was a lowering of blood amino acid nitrogen, which was considered a response to elevated epinephrine levels in the animal. Since there was no qualitatively observable change in any single amino acid as revealed by two-dimensional paper chromatography, it is believed that the fall in amino-acid nitrogen is nonspecific with regard to individual amino acids.

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Nature in her inscrutable wisdom has set limits which she never oversteps.—JEAN REY.

HELMINTHOLOGY.—*A review of the cestode genus* Echeneibothrium.

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This paper is an attempt to clarify the taxonomy of the cestode genus *Echeneibothrium* (family Phyllobothriidae; type species, *minimum* van Beneden, 1850). This genus, described by van Beneden in 1850, comprises at least 18 recognizable species common in selachian hosts. It has been in need of review for some time.

Van Beneden based his genus on material from *Trygon pastinaca* (= *Dasyatis pastinaca*). Prior to his account, however, Rudolphi (1819) had described *Bothriocephalus tumidulus* (= *Tetrabothrius tumidulus*), now recognized as *Echeneibothrium tumidulum* from the same host. He described the genus as having four bothridia borne on long pedicels extremely variable in form. These have regular "replis" over the whole extent of the organ which resemble the "lamelles" on the head of an *Echeneis* whence the generic name. In classifying the species of *Echeneibothrium* these ridges, and the consequent number and arrangement of loculi, are usually considered important; but in view of the statement of Woodland (1927, p. 520) that in *E. maculatum* the ridges separating the loculi are temporary, and that therefore the latter "are often entirely absent or at least invisible," some question arises regarding the importance of this feature in classification.

Echeneibothrium is a very variable genus, which accounts largely for the confusion in classification of its species. Van Beneden (1850, p. 113) says of the bothridia that they are "extraordinairement variables dans leur forme," and Beauchamp (1905, p. 520) says: "Ni le nombre des alvéoles . . .ni la forme générale de la bothridie, ni la présence ou l'absence de myzorhynchus . . . ne sont des charactères au dessus de tout critique. Enfin le longueur du cou et la loi de croissance des anneaux sont charactères de varieté d'individu plutôt que d'espèce. Il en résulte qu'il n'y a peutêtre pas dans le genre une seule espèce qui puisse être nettement distinguée des autres."

This statement is perhaps somewhat exaggerated, but it emphasizes the need for caution in defining and describing species in this genus.

RELATION TO OTHER GENERA

Rudolphi (1819) described *Bothriocephalus tumidulus*, Leuckart (1819) *B. echeneis*, Diesing (1863) *Tetrabothrius sphaerocephalum*, van Beneden (1850) *Tetrabothrium variabile*, Linton (1890) *Rhinebothrium*, Shipley and Hornell (1906) *Tiarobothrium*, and Baer (1948) *Caulobothrium*, all of which are probably synonymous with *Echeneibothrium*. Joyeux and Baer (1936) recognized the synonymy of the latter with *Rhinebothrium*, as did Southwell (1925), but Baer (1948) accepted *Rhinebothrium* as a genus distinct from *Echeneibothrium*, splitting the latter into three genera, viz: *Echeneibothrium* proper, *Rhinebothrium*, and *Caulobothrium*. According to this author (l.c., p. 83) the first of these is restricted to European forms, while the two latter occur in *Dasyatis centrura* from Woods Hole, Mass. In the European species a myzorhynchus is always present, while in the specimens from Woods Hole there is never the "moindre trace" of one. Beauchamp (l.c., p. 520) says the myzorhynchus can be "complètement invisible quand il est rétracté et ne crée pas alors un élargissement particulier de la tête." And Joyeux and Baer (l.c.) speak of a "petit" myzorhynchus in *E. minimum*, *tumidulum*, and *julievansium*, while van Beneden (l.c.) says that in the first of these it is "peu développé." Its value therefore as a specific, much less a generic character, is questionable.

Baer (l.c., p. 98) defines *Rhinebothrium* as lacking testes between the genital atrium and the ovary on the poral side of the proglottid. But on page 94 he apparently contradicts himself by stating that "les testicules . . . . contrairement à ce que nous avons observé dans les autres espèces, occupent toute la face dorsale de l'anneau jusqu'à l'ovaire . . . en arriere de l'atrium genitale et des testicules se trouvent toujours entre celui-ci et

l'ovaire *dans la moitié porale du segment*" (italics are the writer's). In *Caulobothrium* the testes occupy this space. In *Rhinebothrium* the longitudinal musculature is reduced to a single subcuticular layer, while in *Caulobothrium* there is, in addition to the subcuticular layer, a parenchymal layer outside of the vitellaria but within the subcuticular layer. In *Rhinebothrium* the vitellaria are restricted to two lateral bands, while in *Caulobothrium* they encroach laterally on the dorsal and ventral faces of the medullary parenchyma (p. 96). In the writer's opinion these differences are not sufficient to divide the genus *Echeneibothrium* into three genera.

Echeneibothrium approaches most nearly to *Anthobothrium*, and the relationship is strengthened by the fact that in at least one species of the latter genus, *Spongiobothrium variable* (Linton, 1889) (= *Anthobothrium variabile*) the bothridia have a series of incomplete divisions resembling the loculi of the former.

Discobothrium fallax van Beneden, 1871, has been referred by Monticelli (1890) to *Echeneibothrium variabile*, a reference supported by Olsson (1893) but opposed by Lönnberg (l.c.), who maintained the distinctness of the two species.[1] Southwell (l.c.) regards it as an interesting link between the Tetraphillidea and the Cyclophyllidea, but in 1930 he considers *Discobothrium* and *Echeneibothrium* to be distinct.

Yamaguti (1934), recognizing the genus *Discobothrium*, includes in it the species *japonicum*, (which he describes as new) from *Narke japonica*. Braun (1894-1900) considers *D. fallax* a valid species on the basis of Lönnberg's opinion. It is also listed as a distinct species by Joyeux and Baer (l.c.). These authors give the absence of bothridia as one of the characters of the Cephalobothriidae, although they describe the "quatre petites ventouses situées en arrière" of the myzorhynchus which may perhaps represent reduced bothridia. Wardle and McLeod (1952, pp. 283-4) describe "four bothridia (at the base of the holdfast) borne on long stalks and very mobile." They list *fallax* as a distinct species in the genus *Echeneibothrium*, considering the "resemblance between *fallax* and *Echeneibothrium variable* . . . sufficiently close to make the inclusion of *fallax* within the genus imperative" (p. 240). Beauchamp (l.c., p. 521) agrees with Lönnberg, and Woodland (l.c., p. 532) considers "*Echeneibothrium variable* . . . definitely distinguished from *Discobothrium fallax* on anatomical grounds", the latter being renamed *E. fallax* in which the bothridia "usually show but few indications of loculi" (l.c., p. 523). Though Beauchamp (l.c., p. 509) says of the bothridium that "il ne présente aucune espèce d'alvéoles ou de replis transversaux" yet it is a fact that "very distinct loculi are often to be found." (Woodland, l.c., p. 524). Southwell (l.c., p. 336) says that while he was unable to obtain a specimen of *fallax* "there is little . . . doubt that the genera *Discobothrium* and *Echeneibothrium* are different but closely related."

In view of these conflicting opinions the writer considers it best to place *Discobothrium fallax* among genera and species inquirendae.

LIST OF SPECIES

1. **Echeneibothrium flexile** (Linton, 1890)

This species was described as *Rhinebothrium flexile* in *Trygon centrura* (= *Dasyatis centrura*) from Woods Hole, Mass. There are 38–42 loculi in the bothridia which are borne on long stalks and characterized by a hinge in the middle, giving the appearance of two bothridia on one stalk. The bothridial halves face each other when contracted. A myzorhynchus is apparently absent. Testes 10 in number. Ovarial lobes very long "extending nearly to the middle of the segment" (Linton l.c., p. 770).

2. **Echeneibothrium variabile** van Beneden, 1850

This species, from *Raja clavata, R batis, and R. rubus*, is characterized by a definite myzorhynchus. According to van Beneden the strobile reaches a length of 100 mm, with at least 100 segments, but Beauchamp (l.c.) gives the length as only 25–30 mm with many fewer segments. Joyeux and Baer (l.c.) give the number of testes as 20–27, but in figure 14, plate 3, of his

[1] This statement is based on one by Southwell (l. c., p. 335). While Lönnberg lists *?Discobothrium fallax* and *Echeneibothrium variabile* specifically he makes no statement as to whether these are the same or different species and is apparently in doubt about this.

1850 publication van Beneden shows only 16 testes. The cirrus sack is very long (22 mm), reaching the middle of the segment. The bothridia are rather strongly pedunculate, with approximately 8 loculi according to Linton (l.c.), but van Beneden's figure 6, plate 3 (1850), shows 20 loculi. The cirrus is spined according to Linton (1889), or "couvert des mêmes asperités" according to van Beneden (1850, p. 117). Linton considers this species synonymous with *Echeneibothrium sphaerocephalum* of Diesing (1863).

3. **Echeneibothrium tumidulum** (Rudolphi, 1819)
Syn. *Bothriocephalus tumidulus*.

From *Trygon* (= *Dasyatis*) *pastinaca*.

Rudolphi's account of *Bothriocephalus tumidulus* is so brief as to render identification difficult. He apparently had only segments to examine. The bothridia have one long, and several transverse septa and are swollen. According to Dujardin (1845, p. 620) the bothridia are sometimes "puisque contigues, réunies en une masse globuleuse, quelquefois . . . divisés chacun en deux lobes petaloïdes, oblongues et plissés." These differences emphasize the variable form of the worm, any given specimen reflecting the form at the moment of fixation.

Carus (1885) identifies it with both E. *variabile* and E *minimum* and with *Bothriocephalus echeneis* of Leuckart (1819). Southwell (1925) considers it identical with *E. variabile*. Wardle and McLeod (l.c., p. 240) describe it as "an extremely variable form . . . (including) all the non-rostellate forms in which each bothridium has a double series of loculi." They, however, and Joyeux and Baer (l.c.) recognize it as a distinct species, but Baer (l.c., p. 90) finds it "une espèce collective qu'il est impossible de reconnaître, which is the view of the writer.

4. **Echeneibothrium longicolle** (Linton, 1890)
Syn. *Rhinebothrium longicolle*.

This species, from *Myliobatis freminvillei* and *Dasyatis centrura*, is characterized by the large number of testes (150–180) and by the long, well-marked neck, which Baer (l.c., p. 93) defines as a "pédoncule céphalique (whose) structure histologique est bien la même que celle du scolex et non que celle du strobile, (and by) une puissante musculature longitudinale . . . interne du parenchyme corticale . . . in addition to the subcuticular muscles of other species. The cirrus pouch is very long and narrow (0.35–0.36 by 0.037–0.073 mm). In its well-defined neck it resembles *Echeneibothrium urobatidium* (Young, 1955), but as stated there it differs in many important respects from the latter.

5. **Echeneibothrium palombii** (Baer, 1948)
Syn. *Rhinebothrium palombii*.

This species from *Dasyatis violacea* is characterized by the single row of 20–22 loculi, by the size of its cirrus sack (0.36 mm long) and by the number of testes (95–100).

6. **Echeneibothrium maccallumi** (Linton, 1924)
Syn. *Rhinebothrium maccallumi*.

This species, from *Dasyatis centrura*, is characterized by its few testes (4 or 5), although Southwell (1925) considers it synonymous with *E. tumidulum*, which has 20–25 testes according to Joyeux and Baer (l.c.).

7. **Echeneibothrium burgeri** (Baer, 1948)
Syn. *Rhinebothrium burgeri*.

Baer does not give the host of this species, merely stating that it was collected at Woods Hole. According to him it is very near *E. flexile*. The difference in number of testes (30–35 in *burgeri* vs. 14–18 in *flexile*), however, would seem to be distinctive, unless this is a matter of development solely.

8. **Echeneibothrium shipleyi** (Southwell, 1912)
Syn. *Rhinebothrium shipleyi*.

Southwell first reported this as a distinct species from *Trygon* (= *Dasyatis kuhli*). Later (1925) he relegated it to synonymy with *Echeneibothrium minimum* because of transverse septa only in the bothridia. Yamaguti (1934, p. 63), however, "on the basis of material from *Dasyatis akajei* which corresponds to Southwell's original description" held that his species was "unquestionably valid." There is no myzorhynchus or neck. Southwell (1930, p. 215) states that "immediately posterior to the head (there) is a swollen bulbous portion, triangular in shape, with the apex passing into the proglottides."

Since *E. shipleyi* has 130 segments, whereas *E. minimum* has only 15, and has over 40 testes, while minimum has about 20 only, the writer agrees with Yamaguti.

9. **Echeneibothrium cancellatum** (Linton, 1890)
Syn. *Rhinebothrium cancellatum*.

This species from *Rhinoptera javanica* has three rows of loculi in the bothridia which are mounted on short pedicels, two lateral rows and one median, producing about 21 loculi. Southwell (1925) and Wardle and McLeod (l.c.) accept this species as valid, but some doubt as to its status arises from the observations of Woodland on *Echeneibothrium maculatum*. And Linton himself says (p. 772) "I am in doubt as to whether the number of loculi is always constant." But he goes on to say (p. 775) "the difference between the bothria of *R. cancellatum* and *E. tumidulum* is too profound to admit of reconciliation. No median row of loculi occurs in any of the published figures of *E. tumidulum* . . . while in *R. cancellatum* it is very evident." The cirrus pouch extends a little past the median line, enlarging at its interior end where its diameter nearly equals the segment length. The cirrus is heavily spined, especially at the base, where some of the spines are 0.008 mm long by 0.003 mm broad with sharply recurved and hooked ends. In this respect it suggests *E. minimum*, but the arrangement of the loculi in the bothridia is apparently very different. This and the heavily spined cirrus appear to determine it as a valid species.

10. **Echeneibothrium trifidum** Shipley and Hornell, 1906

This species, from *Trygon walga* (= *Dasyatis walga*.), is characterized by the Y-shaped bothridia with the anterior half cleft longitudinally and divided into 9 loculi in each half, while the basal part has 9 also or 27 in all, viz; 9 large and 18 small. The bothridia are stalked and there is no myzorhynchus.

11. **Echeneibothrium minimum** van Beneden, 1850

This species from *Trygon pastinaca* (= *Dasyatis pastinaca*) has been given as the type of the genus *Echeneibothrium* by Southwell (1925) and Braun (1894–1900), although, so far as the writer can discover, van Beneden did not specify any species as a generic type. He described it as 15–17 mm long and so "grêle" that it was with difficulty discernible to the naked eye. It has only 15 segments. The bothridia are divided "en 8 ou 10 lames qui peuvent se séparer au milieu." A characteristic feature are the heavy spines on the base of the cirrus. "Le bulbe, qui est situé au milieu des quatre appendices, est en génerale peu prononcé dans cette espèce. Des plis se forment sur toute la longeuer des bothridies, et se divisent parfois au milieu par un profond sillon."

12. **Echeneibothrium rankini** (Baer, 1948)
Syn. *Rhinebothrium rankini*.

Baer does not record the host of the single specimen on which he based his description further than stating that it came from Woods Hole. He says it most nearly resembles *shipleyi*, both species having a single row of loculi. He considers it to be distinguished "facilement de toutes les autres du genre" (l.c., p. 88), distinguishing it mainly by the size of the cirrus pouch "qui est presque deux fois plus grande chez *R. shipleyi* que chez *R. rankini*." The proportions which he gives in his table on p. 98 however do not bear out his statement. In *shipleyi* the pouch measures 0.4/0.024 mm, while in *rankini* it measures 0.216/0.09. These figures indicate that in the former the pouch was more extended, while in the latter it was more compressed. Were the shape of the pouch in each species similar it is most likely that the size (i.e., volume) in each would be similar. For this reason, and because Baer's description was based on a single specimen, the writer considers it probable that the two species are identical.

13. **Echeneibothrium julievansium** Woodland, 1927

This species, from *Raja maculata*, based on a single immature specimen, was differentiated from E. *variabile* by the shape of the progottids and the presence of two pairs of excretory vessels, which latter fact was, in Woodland's opinion, "sufficient to disprove the identity" of these two species. In *variabile* there is, according to Woodland, only a single vessel, which is unusual in cestodes, the presence of two pairs of vessels being the usual arrangement. This suggests the possibility of a mistake by Woodland in overlooking the dorsal vessels in *variabile*, they being small and in some cases difficult to detect. Joyeux and Baer (l.c.) recognize E. *julievansium* as a valid species which, according to them, is characterized "par le fait que les quatre bothridies sont globuleuses." (p. 87). It is also recognized by Wardle and McLeod (l.c.) on the basis of its suckerlike bothridia. Not having seen any specimens the present writer hesitates to give it a

definite status, preferring to place it in the class of species inquirendae.

14. **Echeneibothrium maculatum** Woodland, 1927

A prominent, permanent "rostellum" (myzorhynchus) is given by Woodland as a characteristic of this species from *Raja maculata*. This has, however, been described for several other species, and, as van Beneden (1850) and Beauchamp (l.c.) have pointed out, the variability of this organ, and the uncertainty even as to its presence, militate against its use as a means of specific determination. The writer finds no character sufficiently definite to determine this as a distinct species, and it is accordingly relegated to the group of species inquirendae.

15. **Echeneibothrium austrinum** Linton, 1924

Linton gives the host of this species as a "large skate (from) Mossel Bay." Southwell (1925) considers it identical with *E. tumidulum*. Linton describes it as having a myzorhynchus and a subglobular scolex with the "bothria united at base, . . . nearly circular in outline and provided with small loculi" (p. 14) indeterminate in number. There is no neck. The cirrus is "armed with closely set spines. Vitellaria massive at side mainly in front of the level of the cirrus pouch, but also continuing to the end of the proglottides" (p. 15). The length of two strobiles was 24 and 32 mm, respectively, while two scolices measured 0.21 and 0.26 mm in diameter. In the writer's opinion there is nothing distinctive in Linton's description and he believes this form should be placed among species inquirendae.

16. **Echeneibothrium insignia** (Southwell, 1911)
Syn. *Rhinebothrium insignia*.

In his monograph Southwell (1925) stated that on reexamination of his material he believed this species from *Trygon* (= *Dasyatis*) *warnak* to be identical with *Echeneibothrium flexile*, but Baer (l.c., p. 95, footnote 1) has pointed out that Southwell's figure 138 is totally different from his figure 139 in the form of the ovary, and especially in the number and distribution of the testes, though both of these figures refer to *E. flexile* (= *R. insignia*). In the light of these uncertainties the writer is inclined to refer *insignia* to species inquirendae.

17. **Echeneibothrium javanicum** Shipley and Hornell, 1906

This species is described by these authors as 9–12 mm long with a neck that occupies one-third to one-half the length of the strobile, pedunculate bothridia divided into 29 loculi by two long, and several transverse ridges, lacking a myzorhynchus and with a cirrus minutely spined. In its three rows of loculi it resembles *E. cancellatum*, both from the same host, *Rhinoptera javanica*, but the heavily spined cirrus of *cancellatum* distinguishes clearly between these two species.

18. **Echeneibothrium hornelli,** new name for **T. javanicum** Shipley and Hornell, 1906
Syn. *Tiarabothrium javanicum*.

This species is described as 11–12 mm long with sessile bothridia divided into 12 loculi by transverse septa only. The neck has a definite collar and the cirrus is minutely spined. It also is a parasite of *Rhinoptera javanica*. Regarding it Southwell (1925, p. 212) says: "the species . . . appears to be indistinguishable from *E. minimum* . . . except that in the former there is a well-developed myzorhynchus", apparently overlooking the characteristic cirrus in the latter species. The description of this species is inadequate for a comparison with others and the writer accordingly refers it to the list of species inquirendae.

19. **Echeneibothrium trigonis** Shipley and Hornell, 1906

In this species from *Trygon walga* (= *Dasyatis walga*) the bothridia are sessile and are divided into 7 or 8 loculi by transverse partitions only. It varies in length from 8 to 15 mm. The "stout, unsegmented neck is a good deal broader than the succeeding segmented parts" (p. 81). The authors do not describe the cirrus or give the testes number so that an exact comparison with other species is impossible.

20. **Echeneibothrium myzorhynchum** Hart, 1936

This species, from *Raja binoculata*, is defined by the author as possessing a well-developed myzorhynchus, pedunculate bothridia with ten transverse loculi and ten or twelve testes located in the anterior part of the proglottid. It has a maximum length of 15 mm, with 64 distinct proglottids. The author states (p. 495) that it "is separated from all species of *Echeneibothrium*

excepting *E. minimum* . . . by the fact that the bothridia are divided only by transverse septa," but his figure 6 shows a longitudinal septum very clearly, with nine pairs of loculi and two at the ends, making 20 in all. In its large myzorhynchus and cirrus sack it resembles *E. variabile* but is smaller (15 mm and 64 proglottids compared with 35–70 mm, according to Joyeux and Baer, and at least 100 segments according to van Beneden in *variabile*). Hart does not give the number of specimens examined by him, so that it is doubtful how comprehensive his data are. Another similarity between *myzorhynchum* and *variabile* is the spiny cirrus.

Whether the differences in size and testis number are sufficient to separate these two species is, in the writer's opinion, doubtful, so that he prefers to relegate *myzorhynchum* to species inquirendae.

21. **Echeneibothrium dubium** van Beneden, 1858

From *Raja batis*. Van Beneden lists the characters separating *E. dubium* from *E. minimum* and *E. variabile* as follows: (1) In *dubium* the bothridia are borne on long peduncles and have few (6–8) areolae; (2) the myzorhynchus is more slender than in *variabile*; (3) the form of the spines on the cirrus is different from that in *minimum* and *variabile*; (4) it occurs in a different host. In van Beneden's figures 3 and 12 the spines on the cirrus of *E. dubium* are shown in comparison with those of *E. minimum*. Both are of about the same length, those of the latter being somewhat stouter. Whether these differences are sufficient to separate the two species is, in the opinion of the writer, doubtful, and he is therefore in agreement with Southwell (1925) who considers *dubium* and *minimum* synonymous, in spite of the opinion of Joyeux and Baer (l.c.) who consider *dubium* a distinct species.

22. **Echeneibothrium sphaerocephalum** (Deslongchamps, 1824)

Syn. *Bothriocephalus sphaerocephalus*.

According to Diesing (1863) this species, from *Raja clavata*, has oval, pedunculate, multiloculate bothridia, with transverse septa and a spherical myzorhynchus. It measures 25–50 mm. No figure is given and, from the brief account, identification is impossible.

23. **Echeneibothrium gracile** Zschokke, 1889

This species, from *Dasyatis clavata*, measures up to 12 mm in length. The bothridia are borne on short, thick stalks and are divided into 14–18 loculi by a longitudinal and several transverse septa. There is a short, swollen myzorhynchus. The cirrus pouch extends to the middle of the segments, which are 30–50 in number. The end of the cirrus is spined. There are not over 25 large testes situated in front of the genital ducts. In the absence of any distinguishing features the writer regards this as one of species inquirendae.

24. **Echeneibothrium tobigei** Yamaguti, 1934

According to Baer (l.c.) this species, from *Myliobatis tobigei*, is very close to, if not identical with *Rhinebothrium longicolle*, the presence of a vaginal sphincter in the latter and its absence? in the former being apparently the only distinguishing? feature. Length of neck (36 mm in *tobigei*, 7 mm in *longicolle*) is another difference, but to what extent this was determined by the state of contraction or expansion in specimens of the two species is uncertain. According to Yamaguti the bothridia have transverse partitions only which would seem to separate it from *longicolle*, but his figure 92, page 61, shows a double row with a median partition. The testis number is 140–160. In the writer's opinion the two are synonymous.

25. **Echeneibothrium ceylonicum**, Shipley and Hornell, 1906

In this species, from *Trygon walga* (= *Dasyatis walga*), the bothridia are mounted on short stalks and have 14 loculi. It has a neck of fair length, and the strobile is 8 to 25 mm long. The gonopore is "median." Southwell (1925) identifies it with *tumidulum*. Since the latter is probably a complex of species it is fairly safe to identify it with any one of a number of others.

26. **Echeneibothrium ceylonicum** (Shipley and Hornell, 1906)

Syn. *Rhinebothrium ceylonicum*.

The second of these species from *Trygon* (= *Dasyatis*) *Kuhli* and *T. walga* is described as having the bothridia mounted on very short stalks "if indeed they exist at all. Each bothridium is divided by a long and several transverse partitions into about 20 loculi. Southwell (1925) places it in *Phyllobothrium lactuca*, the production of apparent areolae being due to the "folding and frilling of the somewhat thickened margin of the bothridium in precisely the manner shown by van

Beneden" in the latter species. These two species, *E. ceylonicum* and *R. ceylonicum*, are, in the opinion of the writer, synonyms and homonyms, and since *R. ceylonicum* has priority over *E. ceylonicum*,[2] the former name must stand for both species which, in the writer's judgement, lack any characteristic features and are therefore relegated to the status of species inquirendae

27. **Echeneibothrium affine** Olsson, 1867

This species from *Raja radiata* and *R. batis* is also, in the writer's opinion, not well enough defined to entitle it to specific rank. It is therefore allocated to species inquirendae.

28. **Echeneibothrium walga** Shipley and Hornell, 1906

One specimen only, which was ripe, was found by these authors in *Trygon* (= *Dasyatis*) *walga*. It was 7 mm long. Each bothridium is hinged in the middle similar to those of *E. flexile*, and each is divided by a long and several transverse partitions into about 24 loculi. While only a single specimen was available for study the hinged bothridia apparently make this identical with *E. flexile*, which is the view of Southwell (1925, 1930).

29. **Echeneibothrium myliobatis-aquilae** Diesing, 1863

Neither Diesing or Wedl (1855) gives any account of this species other than a description of the eggs. Zschokke (1889) found fragments of a worm in *Mylibatis aquila* which he thought might belong to this species, but he could not definitely locate it in *Echeneibothrium*.

30. **Tritaphros retzii** Lönnberg, 1889

This species, from *Raja clavata*, is considered by Southwell (1925) as possibly synonymous with *E. tumidulum*, but a comparison of his figures 132 and 133 shows at a glance the improbability of such a comparison. It may be identical with *Trilocularia gracilis* (Olsson 1869).

31. **Echeneibothrium** sp. Beauchamp, 1905

This species, from *Raja punctata* and *R. macrorhynchus*, is characterized by a large, extremely variable myzorhynchus. In certain aspects it resembles *E. dubium* of van Beneden (1858) but differs therefrom in the number of its loculi and segments. The bothridia are "à peu près semblables" to those of *E. variabile*, but when elongated their transverse folds may disappear (p. 525). The loculi are 6 or 7 in number on each side. The neck, separated from the strobile by a slight constriction, is "très grêle ainsi que le corps." No details of internal structure are given and, in the light of Beauchamp's uncertainty as to its classification and the lack of more adequate data, the writer must relegate it to the list of species inquirendae.

32. **Echeneibothrium simplex** Shipley and Hornell, 1906

In this species, from *Trygon walga* (= *Dasyatis walga*), the loculi are restricted to the borders of the bothridia. Southwell (1925) considers it synonymous with *Anthobothrium, variabile* an opinion with which the writer concurs.

33. **Echeneibothrium bifidum** Yamaguti, 1952

This species is of interest as the only one recorded thus far in teleosts, to the writer's knowledge. He has, however, found specimens of the genus in an *Atherinops* in southern California and in toadfish (*Opsanus tau*) in the Gulf of Mexico and Chesapeake Bay, the latter of which is awaiting description. It seems remarkable that there are not more records of the genus in teleosts, since it occurs commonly in selachians which prey upon the latter.

Yamaguti's description was based on a single specimen from *Trachurus trachurus*. In its 12–13 transverse, and two longitudinal septa in the bothridia it suggests *E. cancellatum*, but the bilobed bothridia set it apart from that or any other species known to the writer with the possible exception of *E. flexile* and of *E. bi-lobatum* Young, 1955.

34. **Echeneibothrium multorchidum** Young, 1954

This species occurs in the stingray (*Urobatis halleri*) in southern California. It is characterized by the number of testes together with the stalked bothridia which are divided into about 38 loculi by one long, and several transverse partitions. There is no evident myzorhynchus. It shows considerable resemblance to *Rhinebothrium burgeri* (Baer, 1948), but differs from the latter in size and in number of loculi (38 vs. 48–50 in *burgeri*). Available specimens of *E. multorchidum* were not ripe, but it apparently is much shorter and has fewer segments than the former.

35. **Echeneibothrium urobatidium** Young, 1955

This species also occurs commonly in the

[2] See Shipley and Hornell (l. c., pp. 71 and 82).

stingray. It is apparently one of the complex hitherto included in the species *E. tumidulum* but differs from the published accounts of the latter in three respects: (1) Form of strobile. In *tumidulum* the anterior proglottids are conspicuously wider than long, while in *urobatidium* the difference is not nearly so noticeable. (2) The number of proglottids is much smaller in the latter than in the former (about 50 in *urobatidium*, over 200 in *tumidulum*). (3) The testes are 10–12 in *urobatidium*, 20 in *tumidulum*. In its distinct neck it resembles *Echeneibothrium longicolle* but differs from the latter in testis number and in other important respects.

36. **Echeneibothrium bilobatum** Young, 1955

This curious little species from *Urobatis halleri*, a description of which was based on a single immature specimen, resembles *E. flexile* and *E. bifidum* in the bi-lobed bothridia. The two lobes however are different in size, which clearly sets it off from either. It is peculiar in the small number of testes judging from the single specimen available, which are arranged in a single line. In this respect it resembles *E. maccallumi* but differs from the latter in its bi-lobed bothridia and in other ways. It is similar to *E. minimum* and *E. shipleyi* in having transverse septa only in the bothridia, but is very distinct from them in other respects. There is no evident myzorhynchus or distinct neck. There were about 25 proglottids in the specimen observed.

37. **Echeneibothrium** sp.

An *Echeneibothrium* occurs commonly in the toadfish reference to which has already been made.[3] The cysts containing the scolices are located mostly around the rectum. The bothridia have transverse septa only dividing them into about 22 loculi and are borne on stalks about one-half the length of the bothridia themselves.

38. **Echeneibothrium octorchis** Riser, 1955

This species, from *Raja montereyensis*, is characterized by its few (7–9) testes and small strobile. It has 10 loculi in the bothridia which are armed, as are the pedicels, with small curved hooks. The cirrus is covered with fine hairs.

39. **Echeneibothrium dolichoophorum** Riser, 1955

This species, from *Raja rhina*, is characterized by its large myzorhynchus and the long, narrow ovarian wings. The bothridia are 10-loculate.

[3] See p. 256.

40. **Echeneibothrium macrascum** Riser, 1955

In this species from *Raja montereyensis* the pedicels are attached to the posterior borders of the bothridia. There is a massive cirrus sack and a long, much-coiled ejaculatory duct.

SYNONYMY

The following table gives the various synonymies found in the literature:

Echeneibothrium flexile:

Rhinebothrium flexile Linton, 1890
Rhinebothrium insignia Southwell, 1911
Echeneibothrium walga Shipley and Hornell, 1906

Echeneibothrium tumidulum:

Bothriocephalus (Tetrabochrium) tumidulus Rudolphi, 1819
Bothriocephalus echeneis Leuckart, 1819
Echeneibothrium variabile van Beneden, 1850
Echeneibothrium affine Olsson, 1867
Echeneibothrium gracile Zschokke, 1889
Echeneibothrium ceylonicum Shipley and Hornell, 1906
Echeneibothrium austrinum Linton, 1924
Echeneibothrium minimum van Beneden, 1850
Rhinebothrium maccallumi Linton, 1924
Rhinebothrium longicolle Linton, 1890
Tritaphros retzii Lönnberg, 1889
Petalocephalus tumidulus

Echeneibothrium walga:

Rhinebothrium flexile Linton, 1890

Echeneibothrium insignia:

Caulobothrium insignia Baer, 1948
Rhinebothrium insignia Southwell, 1911
Echeneibothrium flexile

Echeneibothrium minimum:

Echeneibothrium sphaerocephalum (Deslongchamps, 1824), Diesing, 1863
Echeneibothrium trigonis Shipley and Hornell, 1906
Tiarabothrium javanicum ibid.,
Rhinebothrium shiplyei Southwell, 1912
Rhinebothrium palombii Baer, 1948
Echeneibothrium dubium Beneden, 1858
Anthobothrium ceylonicum Southwell, 1912

Echeneibothrium cancellatum:

Rhinebothrium cancellatum Linton, 1890
Echeneibothrium javanicum Shipley and Hornell, 1906

Echeneibothrium sphaerocephalum:

Bothriocephalus sphaerocephalus Deslongchamps, 1824
Tetrabothrius sphaerocephalum Diesing, 1863
Echeneibothrium variabile van Beneden, 1850

Echeneibothrium variabile:
Echeneibothrium sphaerocephalum Diesing, 1863
Bothriocephalus echeneis Leuckart, 1819
Tetrabothrium variabile (van Beneden, 1850), Diesing, 1854

Echeneibothrium tobigei:
Rhinebothrium longicolle Linton, 1890

Echeneibothrium simplex:
Anthobothrium variabile (Linton, 1889), Southwell, 1925

Echeneibothrium longicolle:
Rhinebothrium longicolle Linton, 1890
Caulobothrium longicolle Baer, 1948

Echeneibothrium ceylonicum:
Rhinebothrium ceylonicum Shipley and Hornell, 1906; *Phillobothrium lactuca* Beneden, 1850

Echeneibothrium palombii:
Rhinebothrium palombii Baer, 1948

Echeneibothrium fallax:
Discobothrium fallax Beneden, 1871
Echeneibothrium variabile (Monticelli, 1890)

Echeneibothrium maccallumi:
Rhinebothrium maccallumi Linton, 1924
Echeneibothrium tumidulum Rudolphi, 1819

Echeneibothrium burgeri:
Rhinebothrium burgeri Baer, 1948

Echeneibothrium shipleyi:
Rhinebothrium shipleyi Southwell, 1912
Echeneibothrium minimum

Echeneibothrium rankini:
Rhinebothrium rankini Baer, 1948

Echeneibothrium austrinum:
Echeneibothrium tumidulum.

SUMMARY

In this paper a discussion has been given of the genus *Echeneibothrium*, its relation to other genera, and a list of species hitherto described, 18 of which are recognized by the writer. The following key should aid in their identification.

Bothridia hinged.....(1) *flexile*
Bothridia not hinged.....A
A. Bothridia lobed.....B
Bothridia not lobed.....C
B. Lobes equal.....(2) *bifidum*
Lobes unequal.....(3) *bilobatum*
C. Bothridia tripartite.....(4) *trifidum*
Bothridia entire.....D
D. Bothridia with transverse partitions only...E
Bothridia with both transverse and long partitions.....G
E. Base of cirrus with heavy spines, 15 segments (5) *minimum*
Base of cirrus without such spines.....F
F. 10 loculi,[4] about 40 testes.....(6) *shipleyi*
20–22 loculi,[4] 95–100 testes.....(7) *palombii*
G. Myzorhynchus large[5].....H
Myzorhynchus absent or small.....I
H. Testes more than 16.....(8) *variabile*
Testes less than 16, ovary with long wings (9) *doliochoophorum*
I. Bothridia with loculi in 3 rows.....J
Bothridia with loculi in 2 rows.....K
J. Cirrus heavily spined.....(10) *cancellatum*
Cirrus minutely spined.....(11) *javanicum*
K. Testes more than 100.....(12) *longicolle*
Testes less than 100.....L
L. Testes 30 or more.....M
Testes less than 30.....N
M. Bothridia with 48–50 loculi,[4] length 8 mm (13) *burgeri*
Bothridia with 38 loculi,[4] smaller (14) *multorchidium*
N. Testes 20–23.....(15) *macrascum*
Testes 10–12.....(16) *urobatidium*
Testes 7–9.....(17) *octorchis*
Testes 4 or 5.....(18) *maccallumi*

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[4] See p. 256.

[5] While the variability of the myzorhynchus militates against its use in classification, it seems to be the best means for this purpose here.

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The practical man is the man who practices the errors of his forefathers.—HUXLEY.

GEOLOGY.—*A hydrologic budget in relation to the climate and geology of the Beaverdam Creek basin, Eastern Shore of Maryland.*[1] GORDON E. ANDREASEN and WILLIAM C. RASMUSSEN. (Communicated by A. N. Sayre.)

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The Beaverdam Creek basin on the Eastern Shore of Maryland was selected for a detailed quantitative study of the hydrologic cycle. The object was to determine the apportionment of precipitation into direct surface runoff, ground-water recharge (and, later, ground-water discharge in the form of surface runoff), and evapotranspiration. The land surface, the soils, and the ground-water conditions are representative of much of the sandy portion of the Atlantic Coastal Plain, and the humid mesothermal climate is typical of the eastern United States.

The drainage basin, covering an area of 19.5 square miles, is relatively flat, consisting of low marine terraces. The ground-water reservoir that is of significance extends from the land surface to depths of 75 to 175 feet below the surface; it is underlain by a relatively impermeable clayey silt which prevents appreciable water loss through leakage into deeper aquifers, or gain by leakage from them.

Weekly measurements of precipitation, total stream runoff, surface-water storage, ground-water stage, and soil resistivity (the latter as a guide to soil-moisture content) were made during a 2-year period, April 1, 1950, to March 28, 1952. Instrumentation consisted primarily of 25 driven observation wells, 12 rain gages, a stream-gaging station, 5 staff gages on surface streams, and 3 soil-resistivity stations.

The hydrologic measurements are summarized in two budgets, a total budget and a ground-water budget. The total budget of the hydrologic cycle is represented by the equation

$$P = R + ET \pm \Delta SW \pm \Delta SM \pm \Delta GW$$

where P is precipitation, R is runoff in streams, ET is evapotranspiration, ΔSW is change in surface-water storage, ΔSM is change in soil-moisture storage, and ΔGW is change in ground-water storage. The last term, ΔGW, is a function of ground-water stage, or

$$\Delta GW = Y_g \cdot H$$

where H is ground-water stage and Y_g is the gravity yield, or yield under transient drainage of the saturated sediments. The quantities ET and Y_g are unmeasured but not entirely unknown. They are arrived at by a method of convergent approximations, presented as one of the contributions of this paper.

The ground-water budget is represented by the equation

$$G_r = D \pm \Delta H \cdot Y_g + ET_G$$

in which G_r is ground-water recharge, D is ground-water runoff, ΔH is the change in mean ground-water stage, Y_g is gravity yield, and ET_G is the ground-water evapotranspiration. The qualtities G_r and D for each were derived from the hydrograph of the mean ground-water stage and from extrapolated ground-water recession curves and base-flow rating curves (obtained by plotting the mean ground-water stage against the base flow of the stream). The weekly difference between the mean ground-water stage, ΔH, multiplied by the gravity yield, Y_g, gives the net change in ground-water storage. The equation was then solved for ET_G. This method is presented as a second contribution.

The total precipitation during the 2-year period (104 weeks) was 82.83 inches, of which 29.88 inches ran off in the creek, 50.24 inches was evaporated and transpired, and 2.71 inches went into a gain in storage. The change in ground-water storage was 2.66 inches, that in surface-water storage, 0.04 inch, and that in soil-moisture storage, 0.01 inch.

Ground-water recharge totaled 42.63 inches disposed of as follows: ground-water runoff into the creek, 21.46 inches; ET_G

[1] Abstract of a paper presented on January 25, 1956, before the Geological Society of Washington.

19.45 inches; and $\Delta H \cdot Y_g$ (*GW*), +1.72 inches (different from figure of 2.71 given above because based on a period of 104.4 weeks instead of exactly 104 weeks).

It is concluded that abundant rainfall and high infiltration rates provide this portion of the Atlantic Coastal Plain with large quantities of water, which are discharged about equally in the form of runoff and by evapotranspiration. Recovery of water discharged by nonbeneficial plants, or as unused streamflow, would permit great expansion of water facilities for irrigation, industry, or municipal supply.

E. D. Merrill

Elmer Drew Merrill was for his time probably the most widely known botanist in the world. At the International Botanical Congress at Amsterdam in 1935 he literally sat in the center of the world's leaders in plant taxonomy and, through his directorship of important botanical institutions, of persons engaged in botany generally. In 1954 he was honorary president of the Eighth International Botanical Congress at Paris. On the occasion of his seventieth birthday he was called the 'American Linnaeus' for the breadth and detail of his mastery of the field of plant classification, for his originality and ability in methodological and administrative work, and for his ready desire to assist his fellow workers the world over in what was then described as an "often astonishingly effective way."

What may be Merrill's place in the history of botany as written a hundred years from now? Will it be his invention of the 'Merrill case' (cf. Torreya **26:** 50–54. 1926), a highly useful storage carton designed during his Manila years for herbarium filing? Will he be remembered for advocating the naming of periodicals with single word titles? Though he did not originate it he certainly traditionalized the practice in *Hilgardia*, *Brittonia*, and *Arnoldia*. Or will workers using the loose-leaf ledger form of *Index Kewensis* in our larger botanical libraries recall the man who initiated this clever device? Certainly those who use the offset reprint editions of hard-to-obtain botanical titles, like those of Rafinesque and Gronovius, will owe him a debt of gratitude. Taxonomists of the Twenty-first Century engaged in tracing types and in writing floras of southeast Asia and Polynesia will be using his commentaries on the works of Blanco, Loureiro, and Rumphius, not forgetting the bibliographies that he, sometimes in collaboration with Dr. E. H. Walker, assembled. Perhaps by that time there will be such urgent need of these bibliographic tools, from the increment of publication down the years, that their now oft-times reluctant financing will have yielded to ready support. Merrill's *Index Rafinesquianus* (1949) will have stood for a century beside essential tools of the taxonomist, and his labor in bringing together all the vicarious names published by that unhappy and confounding naturalist Rafinesque will be appreciated.

It will be particularly interesting to learn the Twenty-first Century's verdict on his most controversial innovation: herbarium inserts of taxonomic literature. Will its "great utility and eminent practicability" have been realized? Will his insistence that to incorporate the references into the herbarium adds a card catalog and a library to the herbarium thus making a single working unit for the taxonomist be gainsaid? In 1937 Merrill declared, "I am convinced that this innovation is one of the most important advances made in herbarium technique in the last few decades." Or will his best known memento be that graceful, feather-leaved, smooth-trunked palm *Adonidia merrillii* named for him by Beccari? With its spectacular bunches of bright red fruits each resembling a plum, this palm is a native of Palawan, P. I., and now grown on the streets of Manila, but only as a novelty in our southern Gulf Coast cities. Perhaps a hundred years from now it will decorate our gardens as a commonplace. Will 'Merrill's palm' be a part of daily speech tomorrow with as slight awareness of its association as 'Johnson grass' is today?

Of the numerous tribe of Merrills of Maine, whose roots took hold on this continent in 1635, Elmer Drew Merrill was one of twins born on October 15, 1876, the last of a family of five children. The four boys milked the cows, planted, weeded, and hoed potatoes, and dragged the fields for rocks, rocks, and more rocks. At Maine State

College (later the University of Maine) he came under the influence of Prof. F. L. Harvey. In 1898 he took his B. S. degree and returned as an assistant in natural sciences the next fall. In July 1899 he accepted the position of assistant agrostologist at Washington under F. Lamson-Scribner. It seems from the frank autobiographical sketch that Merrill has left us (Asa Gray Bull., n. s., **2:** 335–370. 1953) that Lamson-Scribner's confidence in him far exceeded his own. Though he accomplished a good deal at his government job and might well have remained in the service in Washington, the opening of the Bureau of Agriculture in Manila enticed him to take a position as botanist there. Again he was strongly encouraged by Lamson-Scribner. He left New York on February 22, 1902, for Manila to remain, with few respites, for 22 years. During those years he explored all parts of that archipelago, as well as parts of Java, Borneo, the Malay Peninsula, and southern and eastern China. Merrill described approximately 4,000 species of plants based principally on this experience. The quintescence of these years went into *Plant life of the Pacific world* (1945), a book which Fairfield Osborn aptly described as "an extraordinary accomplishment."

Merrill was always alert to weeds. When he arrived in Manila he found very few plant collections at hand for comparison with the specimens he brought back to the laboratory. He set about collecting the weeds of the Bureau of Agriculture grounds, only to discover that in many instances the endemic plants of southeast Asia were much better known taxonomically than these shifting populations of the sidewalks. Years later he carried his study of immigrant plants much further, checking the collections made on Cook's voyages in the South Pacific to determine the date of introduction of these weeds. In large part his quest was successful, and his last book, *The botany of Cook's voyages* (1954), musters his data in highly readable though somewhat repetitious chapters, and documents the proposition that "pre-Columbian civilizations in America were based absolutely and wholly on a strictly American agriculture, which in turn was based wholly on native American plants." Merrill wrote persuasively, and in this book as in the 23 essays gathered by Dr. Verdoorn and published in 1946 as *Merrilliana* (Chronica Botanica **10:** 127–394) his skill is attested. The plant geographer and ethnobotanist cannot afford to ignore them.

Detailed accounts of his life will appear, but there remains a facet of Dr. Merrill's life that may slip away as those who knew him personally follow him into history: the warm enthusiasm he lived for our enticing science. This was both an international and a personal quality, as the Dutch botanist Dr. Lanjouw has pointed out, and endeared him to a wide circle of men. If he believed a project was a worthy one, he spared no time nor effort actively to push for its completion. Writing hundreds of letters of recommendation sponsoring candidates for fellowships, grants-in-aid, and positions far and near, imaginatively arranging funds for a graduate student to make a field trip that he appreciated so well would stimulate years of productive botanical work, making introductions for distant workers who but for his interest would have continued to work in isolation—these were some of the ways that Dr. Merrill implemented his spirit of service in science. However, it would not be factual to omit he did nettle some of his associates: I remember Professor Jepson's wrath when, in his bumptious enthusiasm, Merrill broke into a supper party that Jepson had planned for a few select friends at a small pub on Kew Green. His professional colleagues, not a little piqued with envy at Merrill's tenacious work habits, gave slight support to many of his expansive plans as being unrealistic and too meddlesome in the research programs of others. Anyway, innovators are irksome. 'To get-the-job-done' philosophy as often as not forgets the cushion in the chair.

The governor-general of the Philippine Islands, Leonard Wood, wrote a judgment of Merrill in 1924 which may well stand: "You have done first class work in everything you have attempted and have gained the confidence, respect and support of those with whom you have come in contact." After a full year of conferences and research abroad in 1951 he slowed down appreciably. He felt he "really should finish certain projects without too much delay." "And yet," he wrote, "I am not ready to agree entirely with the last sentence of Thackeray's *Vanity Fair*: 'Come children, let us shut up the box and the puppets, for our play is played out.'"

JOSEPH EWAN

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